EDITORIAL NOTES.

Mr. Harry Olver, F.R.C.V.S., has been elected Mayor of Tamworth for the year 1889, and we confidently look forward to his having a successful and prosperous year of office. Mr. Olver is still a young man, but his personal qualities seem to have won appreciation wherever he is known. He obtained the Fellowship Degree in 1878, and at the last election of Examiners was elected to a seat upon the Board. Tamworth has honoured the Veterinary Profession before in the person of the late Mr. Robinson, who three times filled the office of Mayor. In no former period of the history of Tamworth, as a Corporate town, has so young a Councillor had the proud distinction of Mayor conferred upon him. No doubt exists, however, as to Mr. Olver's fitness for the position. His election by the Council was unanimous, and those who knew him best are most confident that he possesses all the qualities necessary to assure success, and to fulfil the high expectations of his fellow townsmen.

The Veterinary Profession is always proud of the social elevation of its Members; the advancement of the individual must necessarily add to the dignity of the collective body. The recognition of personal worth is doubly pleasant when we know, as in this case, that it is so well merited. The local press speak in the highest terms of the new mayor, and we cannot refrain from quoting a paragraph from *The Tamworth Herald* of November 10th, which says: "Only those who have filled the office of Mayor are aware of the manifold duties and responsibilities devolving upon the chief civic officer of even a small borough like Tamworth. Considerable inroads are made upon his time, liberality, and energy, and unless he be a man of tact and ability, the chances are very much against his giving satisfaction. Mr. Olver is the youngest member of the Council, and was first elected twelve months ago, but during the short time he has been in the Council Chamber he has given evidence of ability, energy, and business-like tact, and there is every reason to believe that he will uphold the dignity of the Council and the town throughout the ensuing year. His work will be by no means light, because at least two great improvements will be completed during his mayoralty. The first of these is one with which he was so closely identified, namely, the new Municipal Buildings, and the other that of the Infectious Diseases Hospital. These two improvements alone are sufficient to invest Mr. Olver's term of office with special importance."

The proceedings of two Societies have this week occupied so much space in our little journal as to crowd out several interesting articles and letters which will appear in our next issue. Should we at any-time cut down the essays sent us, we trust the authors will accept our excuse that until we are able to enlarge our columns we must do our best to make them as readable as possible. The discussions on the papers we do not intend to condense. They are often the most practical and instructive part of the proceedings, and are usually reported much too curtly.

We are always thankful to the Secretaries of local Societies for reports of their meetings, and only too glad to announce the date and subject of all coming meetings. Twice lately we have received announcements on Friday mornings, and as our subscribers' copies are posted before 5 on Friday afternoons they were, of course, too late for insertion in the current number. These announcements should reach us by Wednesday morning, otherwise our printing arrangements may compel their being held over till the following week, by which time it is often too late to be of any use. Want of space, not delay, has this week crowded out some notices—one of which however we can just manage to put into this column.

Mr. H. Kidd writes us that The Royal Counties V.M.S. will hold their next meeting at the First Avenue Hotel, on Friday, 30th November, when a discussion is to take place on the paper by Professor Walley, on Ophthalmia, read at last meeting. Luncheon at 1.30 p.m. Chair to be taken at 2.30 p.m.

CLINICAL NOTES.

SCROTAL HERNIA IN THE FOAL, "THE COBBLER'S OPERATION."

Some years ago, I received a very urgent message to attend a foal that was "very bad with a griping." The little sufferer, a horse foal about three weeks old, I found in considerable pain—hurried breathing, constant change of position, lying down and getting up, movement of the tail, copious perspiration, intermittent and spasmodic efforts to suck the dam, rather as an effort to obtain relief than from a desire for nourishment. I was informed it had been doing well up to two or three days before, when it was first noticed to be ill, that it was gradually getting worse, and that it had been given two or three colic draughts without any benefit.

I ascertained the cause of illness to be Scrotal Hernia, and that the abdominal rings were so enlarged as almost to admit the passage of the closed hand. Taking all the circumstances into consideration, I concluded that an operation would be the only means of permanently relieving the animal, and I determined to put to the test a method that had been described to me by a friend who had seen it performed by a country shoe-maker (who by the way, is nothing loth to drive his awl through living skin as well as dead), and, therefore, I have named it, not inaptly I think, "The Cobbler's Opera-

tion." The operation was eminently successful, and as I have not seen any reference to it by any Veterinary authority, I send a description for the benefit of the readers of "*The Record.*"

Provide ordinary hemp thread, such as is used by harness makers, a curved surgical needle, and some cotton wool. When the animal is cast and secured and the hernia reduced, gather up the skin from the scrotum to the stifle on either side, which is then held by an assistant while the operator inserts a line of sutures almost from stifle to stifle.

With a little cotton wool a not too bulky knot is made on the end of a double thread of hemp, beginning near one stifle, the needle is passed through the skin and the knot drawn home, at a distance of about one inch the needle is passed back, but before the thread is drawn tight, place a little cotton wool between it and the skin; and so the needle is passed through from side to side marking inch spaces, with cotton wool between the thread and skin in each instance, until the opposite stifle is reached, when, before securing the end of the thread the whole is well, but not too severely, tightened.

It will be noticed, that as only half of the skin will be under pressure, and is protected from contact with the thread by the cotten wool, there is no danger of sloughing. The hemp rots in about ten or twelve days, liberating the skin. No after treatment is necessary, unless the knot at either side has been too large, in which case there would be abrasion of contiguous surfaces. The testicles descend in the ordinary course, and the colt may be castrated in the usual way at the usual time.

A CURIOUS LAMENESS.

By Prof. W. O. Williams.

Some time ago, when seeing several cases at a farm in the neighbourhood, I was casually asked to look at a cob—which was in a box, idle, as during the last few days it had been noticed lame.

On being brought out, it walked several paces quite soundly, but all at once became so lame that it stopped walking and lifted up its off fore leg as if in great pain.

After standing for a minute or two the pain passed away, and the foot was placed on the ground. On again being moved, in a few steps the same symptoms were shown.

The only cause that could be assigned was, that about a week previously the cob had been in a stable, and that during the night a mare broke loose, and in the morning the cob had several scratches on it, but was not lame.

On grasping the affected limb by the fore-arm, and extending the shoulder, the animal was so pained that it almost reared over, but yet in manipulation of the shoulder and elbow, with the other foot held up—neither flinching, swelling, nor heat could be detected. On passing to the knee, and flexing it, a curious symptom was noticed, and that was, that if the limb below the knee before and during flexion was abducted—the painful symptoms were at once exhibited, but that if it were pressed inwards, or adducted, before and during flexion, no pain was experienced.

On examining the front of knee, heat could be detected, and on again flexing the knee and abducting the lower portion of the limb, the Extensor Metacarpi Magnus was found to dislocate itself inwards, and hence the pain.

Diagnosis—Injury to the sheath and annular ligament.

Treatment—Cantharides blister round the joint.

Result—Recovery in a month.

FATTY *SUBSTITUTION* IN THE DORSAL MUSCLES OF THE PIG.

By M. Hedley, F.R.C.V.S.,

Chief Inspector, Veterinary Department, Dublin Castle

Some time ago, my attention was directed to a side of bacon which was evidently abnormal in anatomical construction. The dorsal spinal column was curved downwards, and was surrounded by an excessive and unusual amount of fat. A close examination revealed that besides the curvature, the *tranverse processes* of some of the vertebræ had been fractured and had subsequently united. There was not any evidence to indicate that the spinal cord had been involved at or subsequent to the injury, but it was probable that some of the spinal nerves were affected either at the time of the fracture, or by the growth of the ossific deposit at the points of union.

The deformity was localised, not being more than about seven inches in length. The muscles anteriorly and posteriorly to this were normal in appearance and to the touch.

A section was made through the fattest portion of the injury. The appearance presented was that the outlines of two muscles could be easily traced, but they contained an abnormal quantity of adipose tissue scattered amongst the muscle substance.

I had two slices cut at some distance from each other. In one part the Longissimus Dorsi which forms an approximately circular section of about two inches in diameter was changed into fat. It was only by the closest examination that any trace of muscle could be detected. The aponeurosis which covered it remained, and was most defined towards the antro-inferior portion. This muscle, or rather what should be muscle, was rather deeper in its shade of colour to the surrounding fat. Other muscle bundles were in the section and more easily detected because the muscle fibre was more abundant, while in the anterior aspect of the anterior slice some of the muscles presented a normal appearance.

This condition is not common. I have examined carcases of several hundred swine but have seen but one other case where the muscle appeared to have disappeared and fat to have occupied its place. I have not found any reference to this pathological change in any veterinary work.

The two slices were submitted to my friend Dr. J. Alfred Scott, F.R.C.S., Professor of Physiology and Morbid Anatomy, Carmichael College, Dublin.

His report and remarks are as follows :—

Microscopically the parts which were formerly muscle were almost entirely composed of normal adispose tissue, with, scattered here and there, small bundles composed of one or more muscle fibres. These muscular fibres were apparently all that remained of the original large mass of muscle. Each fibre, at some part, generally presented a more or less normal appearance, but they gradually became narrower and finally dwindled down to a string of connective tissue. The transverse striation could be well seen in the normal parts and could be traced downwards towards the atrophied portion gradually getting fainter as the other characteristics of the muscle fibre vanished.

The masses of tissue visible after the muscle characteristics are lost contain, generally, an unusual quantity of nuclei, which, probably, were those of the connective tissue and capillaries as well as the sarcolemma of the muscle.

The sections cut from places where the fat was not so well marked to the naked eye showed perfectly normal muscle fibres, which at some parts were narrowed by fat cells which lay close to and between them. The fat cells were arranged in elongated groups of from one to a dozen or more cells.

The accompanying drawing shows the connection between the muscle and fat cells :—

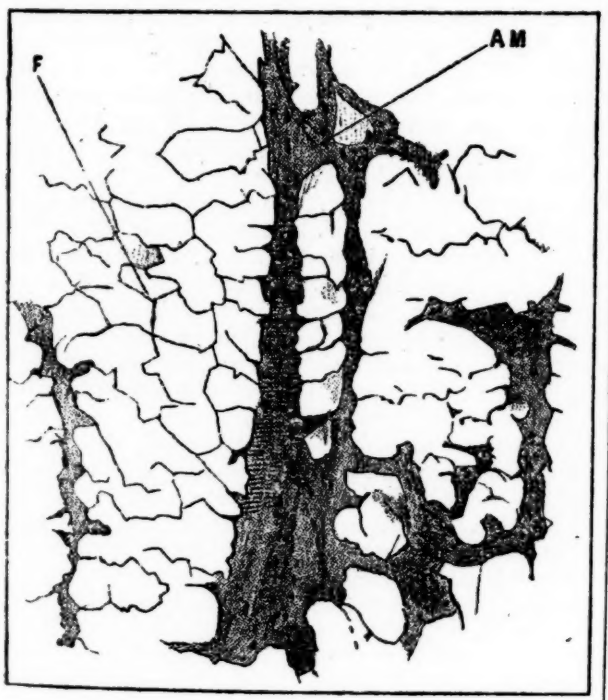

FIGURE I.

F—Adipose tissue or fat. A M—Atrophid Muscle.

On looking up the subject, I can only find very ambiguous references to such a condition in Human pathology.

Sutton* quotes Otto, who wrote about 1831, "That parts devoid of nerves are exceedingly prone to undergo fatty degeneration," and mentions a case † where the spinal cord of a fœtus was damaged in utero during the fifth month of pregnancy, and at birth the lower extremities were masses of fat. From the description, I am inclined to think that this is a similar case, but he gives no description of the microscopical appearances on which the whole argument turns.

Again, Ziegler § states, organs like muscles which for any reason are left unexercised, and so fail to undergo an adequate amount of tissue change, are very apt to become fatty." But this is written in connection with ordinary fatty degeneration.

Corvil and Ranvier || state, "In infantile paralysis and progressive muscular atrophy, the muscular fasiculi totally disappear in consequence of fatty degeneration."

In order to distinguish this condition from some of those cited above, I have called this a fatty substitution, as the terms degeneration and infiltration imply processes different from that which appears to have taken place here, and although both these conditions, the latter particularly, are present, the sequence of events in their production does not appear to be the common one.

The first cause was undoubtedly nerve injury, on account of this the muscle was then probably paralysed, inactive, and wasted, this wasting being caused by the non-growth of the muscles from injury of their trophic nerves, and also by the absorption caused by the pressure of the large quantity of fat deposited in the connective tissue between the muscle fibres. This growth of fat would also be influenced by the feeding which pigs receive a short while before they are slaughtered, as well as the hereditary fatty diathesis in the modern breeds of pigs.

If any true degeneration occurred, some oil granules would be visible in the fibres. In some of the sections where the substitution had gone on to a considerable extent, a muscle fibre had some suspicious granules which may or may not be fat, the difficulty of deciding being intensified by the method of cutting the sections, which was found necessary, from the very fat condition of these parts. I have, however, cut sections in a different manner from portions exhibiting somewhat earlier stages of the disease, and stained them with osmic acid; in these sections, I was able to see clearly the fat cells in the inter-fibrillar connective tissue, and that the fibrils were compressed thereby, but there were not any fat granules in the fibrils themselves.

* Introduction to General Pathology. p. 85.

† Med. Chir. Trans. lxviii. p. 393.

§ General Pathological Anatomy I. p. 81.

|| Manual of Pathological Histology I. p. 443.

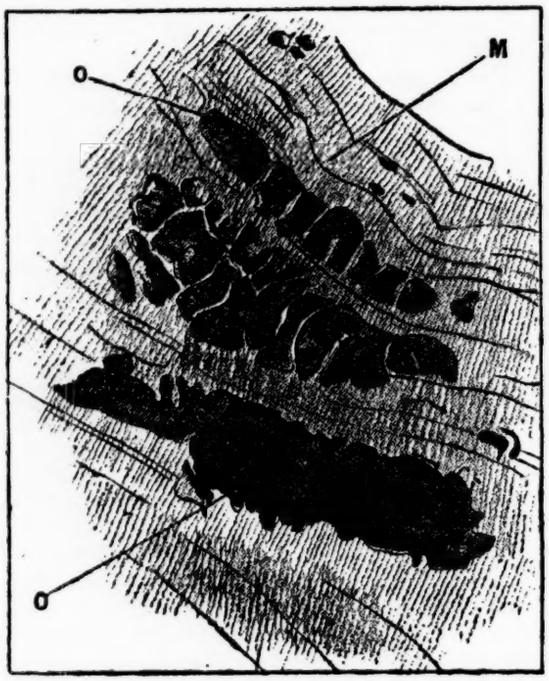

FIGURE II.

O—Adipose tissue or fat stained with Osmic Acid. M—Muscle.

Allowing that the few instances I have mentioned were true degeneration of the fibres, and considering that they were only visible in more or less atrophied portions, I think the true explanation is that some of the last portions of the abnormal protoplasm have been removed in this manner, but their absence in the portion where the disease was commencing shows that it was not the primary lesion.

Fatty infiltration would not be sufficient to cause the absorption of a healthy muscle, and, indeed, it will be seen that some of the muscles in the neighbourhood are apparently normal, probably on account of their different nerve supply.

VETERINARY MEDICAL SOCIETIES.

BORDER COUNTIES' VETERINARY MEDICAL SOCIETY.

The last meeting of the Border Counties Veterinary Medical Society was held at the Bush Hotel, Carlisle, on the 26th of October. The President, Mr. Bell, Carlisle, occupied the chair, and there was a large attendance of members and visitors.

The minutes of the previous meeting were read and confirmed. The President proposed Mr. Rutherford, Edinboro', as a member, which was seconded by Mr. Howe and carried unanimously. Mr. Thompson proposed and Mr. Greaves seconded that Mr. Faulder be elected an honorary member, and was carried unanimously.

The election of office bearers was next proceeded with. The President proposed that Mr. Donald, F.R.C.V.S., of Wigton, be elected President for the ensuing year. He said that Mr. Donald was one of the first to promote the formation of the Society and had earned the honour of being their next President. Mr. Thompson seconded, and said that Mr. Donald was first to suggest the Society's formation, and he had worked hard for it—carried unanimously. Mr. Bell, Mr. Robson, Mr. McConnell and Mr. Clough were elected Vice-Presidents. The Treasurer (Mr. Thompson) and the Secretary (Mr. Armstrong) were re-elected.

Mr. Armstrong then gave notice that at next meeting he would move, that Rule 4 of the Society's rules, be struck out.

A letter was then read by the President from Mr. Hunting, stating that through indisposition he was unable to be present; but had forwarded his paper by train. Mr. Bell then read the paper:—

HEREDITY—

ITS INFLUENCE ON CONFORMATION AND DISEASE,

By W. Hunting, F.R.C.V.S.

It may have occurred to some of you that a practitioner from London was hardly the right sort of man to come to the Border Counties to read a paper on this subject.

You all have experience of animals from their birth upwards; you have personal knowledge of their parentage; you are also enabled to directly observe the similarities and variations in form between parent and offspring. I, on the contrary, have little knowledge of stock under four years old, and still less of their parentage. If my duty here were to afford you information, I should certainly be a pretender; but I assume no such office. The duty I shall attempt to fulfil is simply to open the discussion on a subject of special importance, and, just now, of prominent interest to our profession.

I shall attempt to arrange the subject for you; to be suggestive, and to draw from the store of observations you possess, the facts and opinions so much wanted to form reliable knowledge on the transmission of disease from parent to offspring. I have no new facts for your consideration. I am going to theorise, but I venture to submit that in all pathological or physiological studies, fact and theory are both necessary. Progress is made by a combination of inductive and deductive methods. Your facts and my generalisations may, if guided by the exercise of a little common sense, add something to our knowledge of the subject.

I see no reason to doubt that precisely the same laws are in operation in the transmission of disease, as in the propagation from parent to offspring of conformation, or those less tangible qualities of temper, action, manner and habit. If, then, we first refer to the general laws affecting heredity, we shall be in a better position to consider the special subject of transmitted disease.

The great law is that "like begets like," the truth of which is daily evidenced in the reproduction of type by animals of all sorts of recognised breeds. The offspring resembles *both* parents, partaking more or less of the form and character of each. I say *more or less* because we do not find, in practice, that the inherited characteristics are an exact aver-

age of those possessed by both parents. We do not, for instance, find that if one parent be sixteen hands high, and the other fourteen, that the offspring is fifteen hands; or that if one possess a smooth coat, and the other a curly one, that the progeny exhibit a coat just between the two.

There are certain influences to be considered, which modify the general law.

It is acknowledged that the longer the duration of a peculiarity in a breed, the greater the certainty of transmission.

That the male possesses a greater power of transmitting his peculiarities than the female.

That some parents—either male or female—possess a special power of impressing their peculiarities upon the progeny—this has been called prepotency.

That some animals, instead of showing a resemblance to their parents, show a greater resemblance to some more remote ancestor—atavism, or "throwing back."

There are other influences which affect the inheritance of peculiarities; such as age, food, work, and climate, which may increase or diminish the probabilities of like producing like, and there is one condition which increases them. It is stated by Darwin in these words: "When any peculiarity of structure or constitution is common to both parents, it is often transmitted to the offspring in an augmented degree."

Another general principle which may be accepted is that at whatever period of life a peculiarity first appears, it tends to re-appear in the offspring at a corresponding age or earlier.

Now, bearing these facts in mind, the wonder is that all distinct breeds of animals should show any individual peculiarities. We might almost expect them to arrive, in course of time, at a regular pattern, presenting such slight variation as to be only detectible to the eye of an expert.

There are, however, other agencies at work as well as the master one, expressed in the words "like begets like." The food, the climate, and other conditions of life, produce deviations in structure of animals, which are transmitted. When a variation renders an animal more fit for the surrounding conditions, it soon becomes more pronounced; and when careful selection favors that variation, it is sure to increase. The crossing of distinct varieties also gives rise to deviations.

In the care of our domestic animals then, where we can absolutely control their increase, we have the power, if we only use it, either to vary or to intensify the structural conformation, or even the functional peculiarities of the individuals. In no part of the world is this better known than in this Kingdom, where every one of our breeds of stock stands pre-eminent in those characteristics, which render it most useful to man.

Unfortunately, not only have our animals inherited the valuable peculiarities of structure and constitution possessed by their ancestors, but they have, in a large degree, had transmitted to them some of their infirmities. If our practical breeders had recognised the fact earlier, that some diseases were capable of transmission as certainly as definite forms of structure, we should now have fewer unsound horses. An inherited disease does not obtain the same recognition as an inherited defect of form, for many reasons. It does not appear at once—it only developes after the lapse of some years; and then may be credited solely to some direct exciting cause. Diseases, too, which only appear, as a rule, at four and five years old, may not appeal to a breeder's self-interest, as at that age he has frequently sold the animal, apparently sound, and all trace of its parentage becomes lost by the time defects appear.

It is impossible to discuss the question of hereditary disease, apart from the question of inherited make and shape. There are certain conditions of limb, which undoubtedly predispose to disease; as long pasterns to sprain, certain hocks to curb, flat feet to laminitis, and dry horn to sandcracks. We recognise also a light middle as leading to want of stamina, a long neck as often accompanied by roaring, and even certain shades of colour as indicative of greater or less constitutional merits.

But a special conformation does not always coexist with a definite defect or disease. If it did, the relation of cause and effect would be unmistakeable. If we always found sickle-hocked horses with curbs, and all curbs on sickle-hocks, we should justly draw the inference that one was cause and the other effect. No such clear connection has yet been traced in any of the diseases we are inclined to classify as hereditary.

If we look over a list of them, we shall find that every one of the pathological conditions may arise under circumstances in which it is impossible to impute heredity as a cause, and not infrequently where a distinct exciting cause, of an extraneous nature, can be positively indicated.

An accidental slip may produce a "curb," and "strangles" may give rise to roaring in animals of the very best type of conformation, and possessing parentage of undoubted soundness. We require more facts to enable us to say what diseases are transmissible, and what conformations are likely to lead to serious defects.

That conformation is transmitted, and that some diseases are inherited is beyond dispute. How are we to recognise these diseases? When it can be clearly demonstrated that a disease or defect is the result of accident, what reason is there to imagine it may be transmitted? We know that, as a rule, changes of structure due to accident are not transmitted; and there is abundant evidence that alterations of form resulting from mutilation or injury, are seldom reproduced in the offspring. There are cases, however, in which the opposite obtains. Twisted horns, deformed legs, and blind eyes, have been recorded in the offspring of parents suffering from similar defects due to injury. Against this may be put the cases of mutilation practised by some savage peoples for generations, with apparently no effect in producing hereditary deformity. In the lower animals too, we may point to the "docking" of horses and "cropping" of dogs, repeated through generations, which fails to shorten the tails or ears of the produce of such animals.

Amongst our patients, if not in man, it seems to me that a distinction is to be made between mutilations which entirely remove a portion of the body, and those injuries (or diseases) which merely cause a change of structure. The former are practically never transmitted, the latter sometimes are. Possibly the explanation may lie in the fact of the injured or altered organism in one case remaining, whilst in the other it is removed.

Heredity, it has been said, depends upon the general principle that a vital tendency once established in an animal, continues to run its course not only through that animal, but through its offspring, until deflected by some other cause more powerful—or the same idea may be expressed in other words, thus:—When an organism has adopted a certain line of action, all organisms derived from it tend to follow the same line until deflected. Why an acorn should give rise to an oak, or why equine spermatozoa produce a horse, whilst those of the sheep give rise to a sheep, we know not. We do know, however, that the "vital tendency" of each continues in its own course, and that that course is a repetition of the course run by its predecessor. The deflecting causes we know almost equally little about, but we recognize their existence and that, once in action, the deflection tends to become permanent.

It must not be lost sight of that the "vital tendency" exists in the ultimate cells that compose the tissues of the body, and that it is the state of these that gives rise to all inherited conditions. The cells of the body have each their own function, and in carrying it out exhibit a selective power. Fat cells take up and store fat, pigment cells take up and store pigment. Milk cells secrete milk, and all others—bone cells. muscle cells, &c—have their special function, which is capable of increased or decreased activity. .

No fact is more clearly established than that superior milking and fattening qualities are transmissible. Milk and fat are mere products and the change, induced in the organism, which increases their production must be due to the special cells which produce or store them. By selection then, we can modify the function of the cells of the body—we can increase their activity, and if it is not illogical to suppose that other causes may act deleteriously upon them and cause them to assume the opposite action. If one kind of cell can be acted upon, so can others. The cells of bone may become more or less active; the cells of muscle also may alter in their action, and thus we may have an excessive secretion of bone, or a modification in the action of muscle or nerve. The susceptibility to changes of function may be aggravated by local exciting causes and thereby be increased, or they may not meet with the necessary exciting cause, and then their action ceases, or at any rate, does not show any special activity.

If, for instance, fat cells possess an inherited tendency to functional activity, they can only fulfil their tendency when a good supply of food to produce fat be placed within the animal's reach.

If bone cells possess an inherited tendency to excessive formation of bone, their function may be still further excited by the strain of work to which the animal is put. Is it not probable, that the inherited tendency in both these cases may be retarded or increased respectively, by the supply of food or the effects of work?

This theory, if it be correct, has a direct relation to the hereditary production of good milkers and also to heredity as a factor in the production of ossific deposits. Cells are the living, formative agents of animal tissues, and they alone can receive the impressions which give rise to functional or structural changes.

Only on such hypothesis can we understand how color, courage, temper, and such like characteristics become inherited as well as fattening and milking qualities, and also the more tangible peculiarities of body, head, or limbs.

An animal is a most complex object. Just as an army is made up of a collection of regiments, and these again of many individuals; so an animal body consists of groups of structures which are formed by an aggregation of cells.

The structure and tone of the army is in proportion to the structure and tone of the individuals forming it, and the structure and tone of an animal is in exact ratio to the condition of the cells of which it is constituted. The changes taking place in the unit are at all times important, though seldom very apparent until they, from their intensity or wide diffusion, produce marked effects upon an important part or, perhaps, upon the whole.

The study of the structure and function of the minute organisms, of which animals are composed, was a few years ago looked upon as possessing more interest as a scientific study than value as practical work. Now we know that these investigations are absolutely essential to elucidate the problems of health and disease.

Clinical observation is as valuable as ever, but no disease can be understood if we *stop* at that point. When all our observations are made on the animal and its surroundings, we can only complete the investigation by the aid of the scientist, assisted by all the physical appliances at his command.

Facts and experiences relating to the healthy animal cannot be too carefully and widely collected by the practical observer, but he will never quite grasp their full significance without the aid of the scientific biologist and physiologist.

There is no finality in human knowledge, and only dreamers expect the time to arrive when the why and wherefore of any point in nature will be solved. Our duty is to go plodding on, collecting facts, eliminating errors, and striving for light and truth. Every fact recorded, every error corrected, is something added to human knowledge and progress, and no man amongst us is debarred, be his intelligence and opportunities ever so narrow, from experiencing the satisfaction of the feeling of "something attempted, something done," if he will only watch and note the events of his daily life.

Now, Gentlemen, we come to the practical

question of what diseases are transmissible by inheritance in the horse, and I hope the time I have occupied in preliminary considerations will not be found wasted.

PART II.

There are a number of diseases, which are generally accepted as being of a transmissible nature, and there are some as to which opinion is rather divided. In the present state of our knowledge, it will perhaps be wisest to adopt a discreet scepticism, and make no attempt to classify them. I shall simply ask you to consider them seriatim. In doing so, it will be advisable not to confuse hereditary disease with congenital disease. In the one, an animal comes into the world with some peculiarity already developed, analogous to that distinguishing its parent. The repetition of peculiar marks; the deformities resembling changes of structure in the parent due to injury; in fact, all peculiarites developed before birth, are congenital. Hereditary disease, on the contrary, does not show at birth, but developes in time—what is transmitted is not disease but a *tendency* to disease. The hereditary tendency may exist, but under certain favourable conditions never develope into disease. It is developed with greater certainty and rapidity when some extra exciting cause assists it—see, for instance, the effect of badly ventilated stables upon eyes, work upon hocks, and respiratory affections upon the larynx.

It is true, the line of demarcation between congenital and hereditary disease, is not very definite, but it exists, and I am inclined to class as not hereditary, all those cases of animals being born blind from parents suffering from injured eyes.

Roaring. That this disease is hereditary probably no one disputes, but it does not necessarily follow that a "roaring" parent should always beget a foal which will develope "roaring." As professional men, I hold, our advice should always be that a horse or mare suffering from roaring is unfit for breeding purposes, and this advice may be very positively relied on when either of the parents of such animals are also roarers. That roaring may be induced in a horse with no hereditary tendency to it I am not prepared to deny, and it is possible that in such a case the disease might not be transmitted. This argument is no reason for a Veterinary Surgeon drawing any distinction in practice—the disease is so often hereditary and so serious, that it should always determine rejection in a breeding animal. Of course, the owner of an unsound animal can do as he pleases, and should he be able to afford proof of his animal's innocence, it might be right that individuals or bodies capable of affecting the horse's services should draw the veto placed upon him.

Broken Wind. The late Professor Robertson taught that this disease was to be classed as hereditary. I recognise it as sometimes a cause of sterility in mares—or perhaps it would be safer to say, as sometimes co-existing with sterility—but I have no experience warranting our accepting it as due to any inherited tendency. We want more evidence.

Diseases of the Eye. I consider 'cataract' to be hereditary and also many conditions due to inflammatory action and accompanied by structural changes. Practically, when the outer layers of the eye become opaque, we can only guess the condition of the deeper structures, and a doubt should be given against the horse. Cataract, however small, and all blindness, should lead to veterinary rejection for breeding purposes. The onus of proof of non-transmissibility should rest on the owner.

Splints. No disease is so positively influenced by heredity as this. Their symmetrical position on both legs; their frequent developement without lameness, and their appearance at about the same age in offspring as in parent is strong evidence. Add to this the fact that nearly all our horses show them, that no indigenous breed in a new country does until crossed with European horses, and I think we have absolute proof of their nature. I am told that in the Argentine Republic splints are accepted as evidence of English blood, and that the equine possessor of this little disease at once becomes of greater value. The majority of splints are harmless and therefore only in cases when their position, close to the knee, suggests danger should I notice them in a breeding animal, and then I should not reject unless lameness were also evident.

Spavin. I think there is sufficient evidence of the influence of heredity in causing spavins to warrant us in classing it as a dangerous hereditary disease, and in declining always to recognise a horse with them as fit for breeding purposes. When I say horse I also include mares. I know no special form of hock more liable to spavin than another, and therefore do not accept conformation as a factor in their production.

Curb. I have no evidence as to hereditary tendency in this disease, nor do I consider it of such a serious nature as to warrant us in interfering with the use of either mares or stallions suffering from it. Mr. Banham quotes Dr. Shorthouse as saying "Nine-tenths of the descendants of the famous horse Bird-catcher have a tendency to curbs." This, it will be noticed, does not say that they *develope* curbs, and may be only an opinion. You can hardly appreciate a *tendency* till you see its effects. We want more facts.

Windgalls. I have no evidence of hereditary influence as a cause of this condition, and should certainly not object to an animal for breeding purposes because they were found on its legs.

Bog Spavins. Not sufficient evidence to warrant their inclusion in hereditary diseases.

Thoroughpin. Ditto.

Navicular disease. I think there is sufficient evidence to warrant us in concluding that heredity is a cause of this disease. I should object to the use of a stallion suffering from it. Professor Williams and Mr. Finlay Dun have both recorded facts in favor of this view.

Stringhalt. My experience does not warrant me in classing this as hereditary, though there are one

or two cases recorded very strongly suggestive that it may be.

Shivering. Professor Williams records the case of a mare which suffered from "shivering" and had three or four colts, each of which became affected in the spine before the age of three years.

Epilepsy. Some forms of 'megrims' in the horse are undoubtedly epileptic, and we therefore might assume, from analogy in other animals, that they might be influenced by heredity. We want facts and I know of none.

Sidebone. With the exception of splints, probably no unsoundness owes its prevalence so exclusively to heredity as sidebone. It is the curse of big horses, and should be vigorously attacked by the rejection of all stud animals suffering from it.

Ringbone. I consider ringbone one of the very worst unsoundnesses met with in horses, but I am not prepared to admit that it is a transmissible disease. We require more facts, to arrive at a positive conclusion.

Diseases of the Feet. Narrow feet, wide feet and flat feet are formations undoubtedly transmitted from parent to offspring. Qualities of horn too are transmitted to some degree. Unless disease be present a veterinary surgeon has no right to class these conditions as unsoundness. He may call them hereditary conformations of an undesirable kind, but they are certainly not hereditary diseases. Mr. Banham, in a very able and exhaustive essay on "Hereditary Disease in Farm Animals" refers to the conformation of feet, and quotes from Dr. Shorthouse a remark concerning "Knight of Kars" who had a club foot, "the horse begat 20 or 30 foals a year, and nearly all the foals had a club foot and would not stand training and racing." This does not help us much—it is a case of congenital disease being transmitted, and I think there is more certainty of this than of one which arises as the result of hereditary tendency.

Sandcrack. The existence of a sandcrack in one foot is no evidence of a transmissible disease. If I found a horse with dry brittle hoofs and sandcrack in more than one of them, I should consider the condition one of a clearly constitutional nature, and therefore hereditary.

Laminitis. By this term we mean not only the acute disease, but also the structural changes of hoof produced by it. Mr. Oatway, at a meeting of the Central Veterinary Medical Society, related a case which seems to negative its transmissibility: "A horse named 'Dundee,' had laminitis so badly that he had to be lifted on his feet when a mare was brought to him; another horse named 'Billy Barlow' had it as badly. He knew many, and had owned several of these horses' foals, and in no instance that he knew of had one been affected with laminitis."

Grease. At the same meeting, just alluded to, Messrs. Mulvey and Caton adduced independent evidence of cases of the transmission by a stallion of this disease. Until evidence of the opposite, may be forthcoming, I shall accept hereditary influence as a cause of grease.

THE PRESIDENT—I am simply re-echoing Mr. Hunting's words, when I say that I hope the paper will have the effect of raising a good discussion. Mr. Hunting has covered a good deal of ground, though there are some things he has not touched upon that I should like to have seen mentioned. He then invited the views of the gentlemen present.

MR. GREAVES—Mr. President and Gentlemen, I am afraid that any remarks I may make, will lack practical knowledge; I am, like my friend Mr. Hunting, in a district where there is very little breeding, and I have had very little experience, nevertheless, I have had a little. In the first place, I may say that the impression on my mind as to the conformation of horses, is that colts take their conformation more after the male than the female, and that the intellectual part, or mind, partakes more after the female. I remember that we shod a mare for many years and she had the peculiarity of snatching her feet away from the smith, when being shod. Three or four of her colts had the same peculiarity. Now, in reference to the earliest appearance of defective conformation, I believe that the taint or strain, or whatever you may call it, is produced at the time of copulation. It may never be developed if circumstances do not aggravate it in after-life, but it is certain to appear if the circumstances for its developement are favourable. Last March we had, in Manchester, a paper on "roaring," and since then I have paid a good deal of attention to the subject; I have been in the habit of attending at the knackers, and examining a large number of both roarers and non-roarers, light and heavy horses, and in every case where the muscles of the larynx had been blanched and wasted, I found the recurrent nerve on that side was defective; not a particle of nervous influence had passed to that side of the larynx, and I believe it had been so from the earliest period of gestation. The nearest approach to the larynx would be six or eight inches, and I am of opinion that the nerve had never reached the larynx at any period of its life, but that the muscles of the larynx were in a perfect condition at the time of birth with the exception of the want of nervous influence. I found in the other cases where there was no wasted powers the recurrent nerve was perfect, perhaps as thick as a crow quill and as strong as whip-cord; but the other was as thin as thread, and would break on the slightest tension.

I came here for the purpose of hearing and learning all that I could, for at present, we members of Council have the question to discuss. I think Mr. Hunting is right in the cases he has described—roaring, side-bone, curbs, spavins, and cataract are all hereditary diseases.

MR. YOUNG mentioned the fact of a stallion, which was affected with "shivers," and the disease was transmitted to probably 90 per cent of his foals; and another horse, which had navicular disease and was a roarer also, and both diseases were largely transmitted to his progeny; also two other horses, which were roarers, and both produced a large per-centage of roarers.

MR. ARMSTRONG mentioned two or three horses, which were subject to certain diseases and were

very largely transmitted to their progeny. Also, one horse which had a peculiarity in conformation, and this peculiarity was transmitted to the offspring in almost every case, and any one knowing the horse and coming across his progeny in the district, always recognised them.

Mr. Carruthers mentioned the case of a Clydesdale stallion, which suffered from luxation of the patella, and nearly 75 per cent of his progeny suffered from the same disease.

Mr. Clement Stephenson said that these gentlemen had given the very kind of information that the Council of the Royal College of Veterinary Surgeons wanted. It is not theory at all that is required, we want the very evidence we have heard to-day, and gentlemen who can prove that unsound horses transmit their disease to their progeny, or even that their progeny has a tendency to develope such diseases as soon as exciting causes arise, should afford information. It is not a question of theory —it is facts we want.

Mr. Burnett—We are very much obliged to Mr. Hunting for his valuable paper. He agreed with him that milking and fattening properties were hereditary, if the animals were not crossed by inferior blood. Sometimes heavy milkers develope tuberculosis, and this is hereditary. It one of the most fatal of cow diseases. He had often seen 'shivering' in colts under two years old, and splints and spavins, all, he believed, owing to hereditary influence; also curbs. Sickle-hocked horses have a strong tendency to throw sickle-hocked progeny, and they develope curbs on the slightest invitation.

Mr. Rutherford said, In my mind one can hardly exaggerate the importance of this subject. The Royal Agricultural Society have asked the R.C.V.S. to take this matter up, and we may safely discuss it.

Like our friend Mr. Hunting, I am also a town practitioner, and I certainly have not had the opportunities of country practitioners of watching individual circumstances, which will be of so much assistance to the Council in their enquiry: but it has been my lot to see the horses of a great many countries, and it came to me forcibly when hearing the paper, that in all those countries, there was great relationship between the conformation of the horse and its soundness; and breeders were very careful to use only sires which were the very best types of their kind.

You are all well aware an Arab will not part with a mare, but if one shews any defect she is destroyed. The pedigrees of their stallions are traced for a very long period. I once saw the pedigrees of three stallions which were traced back between 300 and 400 years. These were sent to the Viceroy of India, and they showed undoubtedly that there had never been an impure strain in the breed. It is marvellous the care taken of conformation, stamina, and other qualities which tend to make a good horse, and I think it is the want of this care in this country that has given rise to the anxiety now existing on the question.

Those who have commissions entrusted to them to secure good horses, know of the great difficulty experienced in finding a fair percentage of well-formed, sound horses, with good action and color.

Why is this—Is it not because in many instances the stallions are defective to begin with, and in the large majority of cases the mares were also defective? It was part of my duty, while in India, to proceed to Australia in connection with re-mount work, and I was struck by various signs of Hereditary influence there. We found too, in taking horses to India where they faced new conditions, that the nearer the horse approached to being thoroughbred, the better he met the trying conditions of the country. I have never been able to keep apart hereditary tendency and conformation. I agree there, to a large extent, with Mr. Hunting, I think they go largely together.

When in Melbourne, I was asked in conjunction with another gentleman, to enquire into the cause of a disease occurring in the colony, showing swelling in the head and lameness of the limbs, and swelling of joints, &c. This disease became hereditary, and animals got by stallions affected in this way had a tendency to the disease irrespective of climatic conditions. The principal causes were attributed to the fashion of breeding from colts which had been heavily trained when their bones were not fit to stand the wear and tear. These horses, themselves, had never arrived at maturity, and long before they were half the age of ordinary horses they were sent to the stud.

There can be no doubt about side bone being, as Mr. Hunting says, hereditary. It is almost invariably associated with want of proper form of the feet. There the question comes up again, "Is it the conformation that is hereditary, or is it not?" It is the conformation, I think. I think it is impossible to consider the matter of heredity without taking also into consideration Conformation, Climate, and Soil. There are a lot of diseases peculiar to this country which you do not see in Australia at all. The Australians, 20 years ago, were so particular in their choice of mares, and above all, sound sires. It was exceedingly rare to come across a roarer. The disease is unknown among Arab horses, and out of 2,000 or 3,000 horses met with in Australia, I only saw two or three roarers.

Mr. Thompson—If the Council succeed in getting rid of all the diseases mentioned in the list, Veterinary Surgeons will have very little to do, because most horses had one or more of the diseases which it was intended to find a remedy for. Broken wind is seldom seen in thoroughbred carriage horses, but is comparatively common in cart horses. Curb is certainly hereditary, and he gave instances both of curb and several other defects being transmitted from sire to progeny.

Side bone, I think, is due more to bad shoeing than anything else, large shoes were put on, throwing all the work on the lateral cartilages, causing inflammation, from which the disease developed.

Laminitis I have known in many horses, but I never saw it follow in their foals. He thought the Conformation of the sire descended to the offspring, and instanced several well-known cases.

Mr. Carlisle could not believe that Laminitis was hereditary. Well-formed feet might be affected with inflammation from many causes.

Shivering, he thought, was hereditary, and said, that some horses would transmit it to half of their progeny. There are many diseases classed in the paper which I cannot consider hereditary. He also asked Mr. Rutherford if he ever came across a case of navicular disease in the Arab breed.

Mr. Rutherford—I never found such a case although the horses have the kind of feet one would expect to find it in, long, narrow feet.

Mr. Howe considered from what he had observed that the offspring of animals with laminitis were more prone to have laminitis than others, although you might feed them and treat them as carefully as you liked.

Professor Williams said that he agreed with Mr. Howe, that there was a proneness or tendency in animals to have laminitis whose sire or dam had had the disease, and he was of opinion that by breeding from such animals the disease became hereditary. The tendency is, perhaps, not so great from the sire as from the dam.

Bog Spavins and Stringhalt, Mr. Hunting says, require more evidence. They require no more evidence so far as my experience goes. Thoroughpin is also hereditary, and those of you who live in Scotland and have to do with Clydesdale horses, will very soon have that impressed upon you. There is no doubt that for the last fifteen or twenty years, breeding has gone on from all sorts of horses. Bog Spavins, Shivers, and Roaring are all hereditary. As I am on the Committee of the Council to enquire into this matter, I don't think I should say much more now, I am going to listen to the evidence that is brought before them and I trust the Council will feel it incumbent upon them to consult the whole body of the Profession upon this very important subject.

The President—I can only say in the words made use of here, in respect to the breeding of horses, that like begets like. I have seen some of the very best looking horses and perfectly sound when examined get unsound stock.

I think if breeders had paid more attention to their mares, we should have been in a better position to-day, unfortunately, farmers consider anything good enough to breed from, and nearly all the diseases referred too are hereditary.

He referred to a mare with cataract, which he was called in to examine, and found that she had cataract, he then examined the mother, she also had cataract, and having a mare in his own possession out of the same mare, she was examined also, and found to have cataract, which proved it to be hereditary.

I do not think that Laminitis has very much hereditary tendency, and we should not hastily condemn all cases of Laminitis as such, but I think we can class all the other diseases mentioned as having hereditary tendencies; at all events, I would like to have a Committee of this Society formed to prepare a list of what they consider to be hereditary diseases, and after some discussion, it was decided that the office bearers of the Society form the Committee, to draw up the questions to be put to the Members by Circular, and that they be requested to return the same with their replies.

Mr. Bell then moved a vote of thanks to Mr. Hunting for his very able and excellent paper.

Mr. Greaves said that he had great pleasure in seconding the proposition, which was carried with applause.

Professor Williams proposed, and Mr. Armstrong seconded a vote of thanks to the Chairman, which was carried.

The Chairman having suitably replied, the Meeting terminated, and the Members dined together.

JOHN ARMSTRONG, *Hon. Sec.*

ABORTION.

By E. A. Hollingham.

A Paper read before the Southern Counties' Veterinary Medical Society.

(Continued from page 214.)

Concerning M. Nocard's report to the French Minister of Agriculture, each of you have had the same opportunity of reading it as myself, but in case you have not done so, I may say that he believes it to be a contagious disease caused by the presence of microbes. He contends that the disease has all the characteristics of contagion, and as to what are the precise causes which bring it about, he conceives that three reasonable ones may be considered.

These are: 1st.—Is it a disease of the pregnant cow? 2nd.—Is it a local affection of the genital organs of the mother? 3rd.—Is it a disease of the fœtus, or its envelopes?

The first hypothesis, M. Nocard says, must be discarded, as the general condition of the cow is not affected. The secretions remain normal, and a post-mortem discovers no injury whatever. The second hypothesis is treated by him as deserving further attention, and he has come to his conclusions after full and careful investigations. He bases them, firstly, on the post-mortem examination of cows about to abort; secondly, on the post-mortem examination of cows upon the point of aborting; and thirdly, on the post-mortem examination of cows having aborted the previous year, and not since impregnated.

The following are some of the results of these experiments:—A Nivernaise cow, aged 2½ years, gave premature birth to a fine calf of about five months. His health was good, but she was killed the same day at Nivers. Samples of her blood, milk, and urine were saved, and all the viscera was found normal. On incision into the uterus, there were found some flakes of yellowish mucus mixed with pus. The afterbirth was adherent to the coteledons, and around these was found a quantity of the yellowish material which drains from a cow after abortion. On squeezing

one of the coteledons, a few drops of pus-like matter was obtained, and on scraping the part, a milky-like juice was seen. A small portion of the blood, milk, the coteledon matter, as well as the pus-like matter, was sown in some broth of fowl and veal, and in some tubes of pepto-gelatine. The pus-like matter and uterine mucus was also examined by the microscope, and found to contain large numbers of bacilli and micrococci. After 48 hours, the broth cultivations of the coteledon matter and the pus-like matter had produced exactly the same organisms, whilst the blood and milk showed no organisms whatever. A cow numbered 7 produced the same results as the one just mentioned. Another cow which had aborted several months previously, and had refused the bull, was killed. Her viscera was normal, but on scraping the lining membrane of the uterus, a liquid was obtained which was slightly acid, and swarmed with microbes (living organisms). From his experiments he found that the uterus of aborting cows always contains micro-organisms, while that of a healthy cow does not contain any, neither is the uterine liquid acid.

In all cases of experiments and post-mortems, every precaution was taken to prevent the entrance of foreign germs. From his experiments, M. Nocard draws the following conclusions:—1st, in aborting cows, there exists between the mucous lining of the uterus and the placenta, micro-organisms which are not found in pregnant cows which have previously borne calves, and coming from a district where abortion is unknown. 2nd, the organisms do not seem to exercise any injurious action upon the mucus membrane during gestation or after abortion. 3rd, abortion repeated in the same animal is explained, if the influence of the microbe is admitted, by its persistence in the uterus until the time it can exercise its influence upon a new fœtus, or its envelopes. 4th, continued sterility is also explained by the acid re-action of the liquid in which the microbes perpetuate themselves, the male spermatozoa not being able to live in an acid media. As to the third hypothesis, "Does abortion result from diseases of the fœtus, or its envelopes?" M. Nocard, in order to answer this question, made two series of post-mortems. The first were with aborted calves which seemed likely to live, and the second with those not likely to live. In the first series no micro-organisms were found in the blood, bile, spleen, liver, or lymphatics. In the second, the same results were obtained, but in the digestive organs numerous microbes of various kinds were found; and bear in mind that, as they had not been used, no organisms could have been obtained from without; and again, the digestive system of a normal fœtus under the same conditions does not shew any trace of them.

If we accept the result of these investigations, it will explain how, although the malady is contagious (or rather may be so), it does not spread as rapidly, nor in the same manner as other contagious diseases, for it appears these microbes are only found in the generative organs and fœtal intestinal tract, and that infection only takes place when the virus comes in direct contact with these organs, thus differing essentially from pleuro-pneumonia, or foot and mouth disease. I conceive we ought to be careful not to embrace this theory to the exclusion of all others.

I have given you a list of conditions each of which may cause the complaint, and this list I could make considerably longer. For my own part, I only accept M. Nocard's conclusions as being one probable cause, which I do not think is so frequently in operation as those which result from certain dietetic and other conditions which I have brought to your notice. It is only reasonable to suppose, however, that outbreaks do arise where the active agent is this microbe, and in those cases it is easy to understand the immense importance of cleanliness and a free use of disinfectants. Having regard to the manner of infection, it is clear that collections of manure or urine in the channels at the back of the byres, would offer every facility for the infection of other cows, as a result of their tail and hind quarters becoming soiled, while lying down, with the excreta or discharges which have passed from an aborting animal. Amongst other preventive methods, I would recommend that when an abortion has taken place, the fœtus and envelope be buried four feet deep and covered with lime. The stall or box should then be washed down with very hot carbolised water, and brushes. If the building contains other female animals, whether pregnant or not, the patient should be removed and isolated until all traces of discharge have ceased. The tail and hind quarters should be sponged and cleaned daily with a solution of sanitas, or some of the coal-tar preparations, in water. Keep the box or building exclusively for the accommodation of these cases, and direct that the inmate or inmates shall be milked and fed *after all the others* have been attended to. Of course, all litter and manure should be included in the disinfection process, and then, gentlemen, when we have taken all these precautions it is only by non-recurrence of the complaint that we could judge as to whether this infectious principle had been destroyed or not, for we have no information as to any period of incubation. M. Nocard is of opinion that if a cow is exposed, and becomes infected after the seventh month she will resist the specific action of the microbe until she has calved at her proper time and become again impregnated, but even after that it persists. If this opinion is correct, it is very clear that the only rational treatment for this class of case is the poleaxe.

In dealing with the preventive treatment connected with the other causes I have named, I think it is sufficiently indicated by the description I have given of the various prejudicial influences. I think the great prophylactic measure is to be found in an absence of what I term coddling. The temperature inside buildings should not be 60 to 80 when it is 20 to 40 outside. There is also a great tendency to try and transform cows into mere milking machines, and it is a hard task to make some men understand the dangerous tendency of their en-

deavours. They do a short sum in arithmetic as if they were dealing with a steam engine, so much coal, so much water, and that equals so much power. With a cow, it is so much cake, chaff, grains, roots, meal, and hay, equal to so much milk or so much beef. One important factor in the preservation of health is exercise, especially in regard to diseases of the digestive organs. This is often overlooked, or disregarded. Rather than keep cows tied up week after week during the winter, I have frequently recommended that they should be driven along the high road for a mile or two.

In concluding, I propose saying a word or two concerning the symptoms, and most frequent complications. In most cases which occur from the 3rd to the 5th month, the symptoms are slight, and often result in the dropping of the fœtus without giving time for treatment. Where, however, the time is prolonged, and the premonitory uneasiness and straining are observed, I recommend the removal of the animal to an absolutely dark box, away from all noise or bustle, and the administration of Chlor. Hyd. with Chloroform, or Brom. Pot. or Tinct. Opii. In these cases, the fœtal envelopes generally come away intact, and the health of the mother is apparently not interfered with, so that beyond isolation and disinfection, nothing else need be attempted. There occur, however, cases of abortion and premature birth where the symptoms are very severe and may possibly result fatally. Violent straining without any progress being made with the birth will sometimes produce prolapses of the rectum. The continued rigidity and smallness of the cervix may result in fatal exhaustion. After the fœtus is extracted, the straining may continue in spite of all treatment, and result in inversion of the uterus. As you well know, all these are conditions of considerable gravity and require the practised obstetrist to successfully cope with. These complications, however, are fortunately not common. There is one result which we often meet with—I refer to retention of the fœtal membranes either as a whole, or of some portion. This takes place to a greater or less degree in nine-tenths of the cases of abortion, or premature birth up to the 8th month. In these cases, I prefer to remove the membranes with the hand; that is if it is three or four days since the birth occurred. I am much opposed to allowing the retention of a horribly fœtid mass in the uterus until it practically rots away.

Independent of the risks which your patient runs of Metritis, and parturient septikœmia (and this is not slight), there nearly always occurs a condition of ill-health, which is more or less severe and prolonged according to the intensity of the septic infection. It is not a nice job to remove the membranes, but it may be made very tolerable by the free use of Condy's or Carbolic. My mode is to well wash my arm in a solution, then oil with Carb. oil, insert the pipe of my enema syringe into the uterus and inject a quantity of whatever solution I am using, after which, introduce the hand and carefully lift and break away the retained membrane from its attachments, subsequently washing the uterus out with another pail of the solution. I may say that I always discountenance the use of tiles or other weights attached to the placenta with a view of drawing it away. All attempts should be made by hand, the membranes being very liable to break. If the hand is being used the operator can feel at once if that is taking place, and can then desist. I may say, that if an attempt is made to remove them from outside, the best way I know of applying force is by holding the membrane between two flat sticks and twisting it. This produces steady, equal traction, and is often tried before operating internally.

Lincolnshire Veterinary Medical Society.

ALTERATION OF PLACE OF MEETING.

To suit the convenience of those Members of the Profession residing at a distance, the next meeting of the above Society will be held at the Angel Hotel, Peterboro', on Thursday, November 22nd, at 12 o'clock, instead of at Huntingdon, as previously announced.

Several valuable communications are necessarily held over.

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We had resolved not to publish anything more concerning 'Operations on the Larynx' until some positive discovery had been made, or until some operator had collected a sufficient number of new observations to add to our present knowledge. The lively article in this month's '*Veterinary Journal*' forces us however, in the interests of fair play, to allow two gentlemen space for a reply. Dr. Clarke, as far as we know, has never mentioned Dr. Fleming's name, nor written anything, save an account of his own experiments made months before Dr. Fleming noticed the subject. He therefore did not deserve the smart, but not original sneer implied in the sentence "our efforts appear also to have incurred the disfavour of a surgeon who has ventured to transfer some of his attention from mankind to horses." We also insert Mr. Dollar's letter because having been one of the few individuals with sufficient temerity to criticise Dr. Fleming's work, he has, we think a right to resent his efforts being referred to as "acrimonious criticism" and "puerile nonsense uttered from very unworthy motives."

We certainly should like to know what Dr. Fleming's operation is. Has he perfected an operation, or is he merely experimenting? If the former, his professional brethren really have a claim on his confidence; if the latter his claims are, to say the least, premature. No one wishes to detract from any credit he may justly be entitled to, but the impression prevails that his original operation is not quite a success, and that the great anticipations have not been realised. Certain it is that many gentlemen who have repeated what they believed to be his operation are sadder and wiser men. He either told us too much or not enough. His editorial remark in *The Journal*, that he can afford to disregard criticism "because we had no interests to serve but those of the profession, to the welfare and progress of which we have devoted our life, our energies and our means" is excessively weak. No scientific man can claim exemption from criticism because in some past day he has done good work, nor is it becoming to make wide general professions of personal devotion in lieu of simple statement of facts.

We sincerely trust that Dr. Fleming may be successful in finding a useful operation for the alleviation of Roaring. He has a grand field for experiments. So long as the War Office and the British tax-payer do not object to wholesale operations on the horses of the army, we feel certain the veterinary profession will not. We can only rejoice that so sanguine and enthusiastic a P. V. A. is in a position affording opportunities for pathological and physiological inquiry—opportunities quite out of the reach of ordinary practitioners.

The incision into the larynx is merely the first step of an operation; the whole of the essential work to be done comes after that preliminary, and may be one of at least half-a-dozen different procedures. The exact method of getting at the larynx is practically of no importance compared with what is done when you get there. If Dr. Fleming simply claims credit for introducing to our notice the method of opening the larynx through the crico-thyroid ligament he is entitled to all the merit it deserves, but this reduced claim hardly tallies with his words in the Army Veterinary Report, "Attempts are now being made to abolish this cause of inefficiency (Roaring) by an operation on the larynx which I have introduced."

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deavours. They do a short sum in arithmetic as if they were dealing with a steam engine, so much coal, so much water, and that equals so much power. With a cow, it is so much cake, chaff, grains, roots, meal, and hay, equal to so much milk or so much beef. One important factor in the preservation of health is exercise, especially in regard to diseases of the digestive organs. This is often overlooked, or disregarded. Rather than keep cows tied up week after week during the winter, I have frequently recommended that they should be driven along the high road for a mile or two.

In concluding, I propose saying a word or two concerning the symptoms, and most frequent complications. In most cases which occur from the 3rd to the 5th month, the symptoms are slight, and often result in the dropping of the fœtus without giving time for treatment. Where, however, the time is prolonged, and the premonitory uneasiness and straining are observed, I recommend the removal of the animal to an absolutely dark box, away from all noise or bustle, and the administration of Chlor. Hyd. with Chloroform, or Brom. Pot. or Tinct. Opii. In these cases, the fœtal envelopes generally come away intact, and the health of the mother is apparently not interfered with, so that beyond isolation and disinfection, nothing else need be attempted. There occur, however, cases of abortion and premature birth where the symptoms are very severe and may possibly result fatally. Violent straining without any progress being made with the birth will sometimes produce prolapses of the rectum. The continued rigidity and smallness of the cervix may result in fatal exhaustion. After the fœtus is extracted, the straining may continue in spite of all treatment, and result in inversion of the uterus. As you well know, all these are conditions of considerable gravity and require the practised obstetrist to successfully cope with. These complications, however, are fortunately not common. There is one result which we often meet with—I refer to retention of the fœtal membranes either as a whole, or of some portion. This takes place to a greater or less degree in nine-tenths of the cases of abortion, or premature birth up to the 8th month. In these cases, I prefer to remove the membranes with the hand; that is if it is three or four days since the birth occurred. I am much opposed to allowing the retention of a horribly fœtid mass in the uterus until it practically rots away.

Independent of the risks which your patient runs of Metritis, and parturient septikœmia (and this is not slight), there nearly always occurs a condition of ill-health, which is more or less severe and prolonged according to the intensity of the septic infection. It is not a nice job to remove the membranes, but it may be made very tolerable by the free use of Condy's or Carbolic. My mode is to well wash my arm in a solution, then oil with Carb. oil, insert the pipe of my enema syringe into the uterus and inject a quantity of whatever solution I am using, after which, introduce the hand and carefully lift and break away the retained membrane from its attachments, subsequently washing the uterus out with another pail of the solution. I may say that I always discountenance the use of tiles or other weights attached to the placenta with a view of drawing it away. All attempts should be made by hand, the membranes being very liable to break. If the hand is being used the operator can feel at once if that is taking place, and can then desist. I may say, that if an attempt is made to remove them from outside, the best way I know of applying force is by holding the membrane between two flat sticks and twisting it. This produces steady, equal traction, and is often tried before operating internally.

Lincolnshire Veterinary Medical Society.

ALTERATION OF PLACE OF MEETING.

To suit the convenience of those Members of the Profession residing at a distance, the next meeting of the above Society will be held at the Angel Hotel, Peterboro', on Thursday, November 22nd, at 12 o'clock, instead of at Huntingdon, as previously announced.

Several valuable communications are necessarily held over.

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We had resolved not to publish anything more concerning 'Operations on the Larynx' until some positive discovery had been made, or until some operator had collected a sufficient number of new observations to add to our present knowledge. The lively article in this month's '*Veterinary Journal*' forces us however, in the interests of fair play, to allow two gentlemen space for a reply. Dr. Clarke, as far as we know, has never mentioned Dr. Fleming's name, nor written anything, save an account of his own experiments made months before Dr. Fleming noticed the subject. He therefore did not deserve the smart, but not original sneer implied in the sentence "our efforts appear also to have incurred the disfavour of a surgeon who has ventured to transfer some of his attention from mankind to horses." We also insert Mr. Dollar's letter because having been one of the few individuals with sufficient temerity to criticise Dr. Fleming's work, he has, we think a right to resent his efforts being referred to as "acrimonious criticism" and "puerile nonsense uttered from very unworthy motives."

We certainly should like to know what Dr. Fleming's operation is. Has he perfected an operation, or is he merely experimenting? If the former, his professional brethren really have a claim on his confidence; if the latter his claims are, to say the least, premature. No one wishes to detract from any credit he may justly be entitled to, but the impression prevails that his original operation is not quite a success, and that the great anticipations have not been realised. Certain it is that many gentlemen who have repeated what they believed to be his operation are sadder and wiser men. He either told us too much or not enough. His editorial remark in *The Journal*, that he can afford to disregard criticism "because we had no interests to serve but those of the profession, to the welfare and progress of which we have devoted our life, our energies and our means" is excessively weak. No scientific man can claim exemption from criticism because in some past day he has done good work, nor is it becoming to make wide general professions of personal devotion in lieu of simple statement of facts.

We sincerely trust that Dr. Fleming may be successful in finding a useful operation for the alleviation of Roaring. He has a grand field for experiments. So long as the War Office and the British tax-payer do not object to wholesale operations on the horses of the army, we feel certain the veterinary profession will not. We can only rejoice that so sanguine and enthusiastic a P. V. A. is in a position affording opportunities for pathological and physiological inquiry—opportunities quite out of the reach of ordinary practitioners.

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ARTICLES AND CASES.

ABORTION IN DAIRY STOCK.

By Parlane M. Walker, M.R.C.V.S., Halifax.

When reading Mr. Hollingham's excellent paper on the above subject in "*The Record*," the idea occurred to me that the experience of other practitioners might be of some service to the Profession generally. Assuming that it will meet with your approval, I venture to make a few observations on the subject.

For a long time it has, I believe, been the generally accepted theory that Ergot has been responsible for a great number of such cases, but my experience does not bear out that theory. In some cases no doubt it may be an indirect cause, not by causing strong uterine contractions, but by causing the death of the fœtus, which, in course of time becomes an irritant, and is expelled. These cases are, however, not of frequent occurrence, and certainly do not justify the assumption that Ergot is the chief cause of abortion in dairy stock. Like Mr. Hollingham, I have seen many outbreaks where, from the previous history, it was impossible to trace the slightest influence to that source.

When in practice for some years in an extensive cheese-making district in Ayrshire, I was frequently consulted as to the cause of abortion in the cow. In one case, an owner of seventy cows assured me that from twenty to forty of the lot aborted every season, that he had tried every means that had ever been suggested to him, including, amongst others equally ridiculous, the superstitious practice of burying the first live calf under the doorstep of the mistal. He had also tried an exchange of calves with a neighbouring farmer but all to no purpose, the stock reared from his neighbour's calves falling into the same deplorable habit, while those taken from his farm and reared by his neighbour were no trouble whatever. Upon enquiry, I found that up to the end of the sixth or seventh month of pregnancy he fed his cattle on the best of food and kept them in good condition, when, as he put it, "for economical reasons, and to assist in bringing them all dry at one time," he changed the diet to one of turnips and straw, in neither of which is there any probability of the presence of Ergot.

In this, as in previous outbreaks on other farms which had come under my observation, the symptoms appeared within two or three weeks after the change of food, a fact which fully confirmed my opinion that improper feeding was the cause. As in the other cases referred to, I recommended a uniform system of good, sound, nourishing diet all the year round; which system being adopted, the disease gradually disappeared—giving, in my opinion, another conclusive proof of the dietetic origin of the outbreak. The pernicious system of changing the food at the end of the sixth or seventh month of the period of gestation, although not universal, was very generally followed in the district, and I may here mention as a remarkable fact that, in those large cheese-making farms where this system was in operation, abortion was prevalent and milk-fever rare; whilst on milk-selling farms in the same neighbourhood, where the stock were kept well up in condition by a uniform system of judicious feeding, abortion was rare, and milk-fever of frequent occurrence. I might enumerate many more cases, but as the conditions were very similar, the treatment the same, and the results equally satisfactory, it is, perhaps, unnecessary.

Some few years ago, my friend, the late Mr. Simpson, of Kendal, asked my advice on the subject. when I recommended the treatment I had practised with so much success, and in the last communication I had from him, shortly before his death, he referred to the highly satisfactory results following the adoption of the method.

Since writing the foregoing, I have been consulted by a stock-owner in this district as to the cause of abortion in five of his cows, and how to put a stop to it. He has decided to act on my advice, and I have no doubt the result will be satisfactory. The cause in this case I believe to be due to forcing (over-feeding).

A FATAL ACCIDENT in the HUNTING FIELD.

REMARKABLE INJURIES.

By H. Olver, F.R.C.V.S., Tamworth.

On Wednesday last, Nov. 14th, I was a witness to the whole of the following facts, which appear to me in many respects extraordinary; and as such, may be worthy a small space in *The Veterinary Record*.

The A hounds met at S. B.; a fox was found almost immediately. A few fields only from the cover, a gentleman riding a horse, the property of W. F. Esq., fell whilst attempting to negociate a fence and wide ditch (on the off side), the mare rolling on to her back in the ditch. A few minutes, with willing hands to assist, saw her on her feet again, apparently none the worse for the mishap—this would be about 11.30 a.m.; from this time to about 2.30 p.m. we had slow hunting (pottering about) but both trotting and galloping was necessary, with an occasional fence; the whole of which the mare went through. Her rider then handed her over to the groom, saying she was going "sticky." The groom followed about a mile farther, in the wake of the field, and finding the mare got worse, requested my attention—this was about three o'clock. A cursory examination satisfied me that the mare was very ill; so I directed the groom to dismount, and walk the mare straight home, six miles or more. I also returned home—and then drove back to see the mare in the stable, which she reached about five o'clock; a few minutes before my arrival. As soon as I saw her in her box, I said she could not live half an hour: she died in less.

The following day, I made a careful *post-mortem* examination; with the subjoined result:

On opening the abdominal cavity, a small quantity of blood was found free. The intestines were healthy, but lying "all of a heap," their attachment

to the mesentery having been torn away, almost throughout; the latter hanging loose. There was superficial rupture of the liver about two inches long. The ligamentous portion of the diaphragm was torn, or rather split; but not separated from its attachments, so that no protrusion of the intestines had taken place. In the cavity of the thorax there was a larger effusion of blood than in the abdomen. The pericardium was ruptured to a length of about three inches, which when removed from its inferior attachments allowed the heart to protrude. Added to which, was a fracture of the seventh rib close to the spine.

After the accident, which occurred about 11.30, there was no fall or other mishap, so that I think there can be no doubt the lesions named must have taken place then: almost any one of those lesions in an ordinary case would be considered sufficient to account for death. Yet with the injuries named, this mare went on in the hunting field (certainly at no great pace) for three hours, was then dismounted for a second horse, and ridden for half an hour more by a groom; afterwards walking six miles home, before succumbing to her frightful injuries.

The history of the case, the whole of which was under my own cognizance, renders it, in my eyes, particularly interesting; as I could scarcely have conceived it possible that an animal could have lived and undergone such exertions whilst in such a condition: and this is my excuse for sending it for publication in *The Record*. I may add that the mare (a well bred one) was about twelve years old, and in every respect healthy—excepting a somewhat weak heart.

EXAMINER AND COUNCILMAN.

By a F.R.C.V.S.

The discussion on the dual appointment has been again revived, and perhaps you will allow me space for a few remarks, I cannot add much to that which has been already advanced against that appointment. I expected to have seen this subject more freely discussed than it has been. Probably it is engaging the attention of many in the profession, although few have yet ventilated their opinions.

I have no doubt many of the Members of the profession are taking less interest in this subject than they would otherwise do, owing to the fact that only Fellows are eligible for election to the Council and Examining Boards. This is very much to be deplored, and in my opinion the sooner Clause IX is repealed the better it will be for the profession. At the same time, it is to be regretted that many who are quite capable of obtaining the Fellowship Degree, for reasons of their own, will not come forward to help to advance the profession for which that Degree was instituted. Still, I consider it prejudicial to the interests of the profession that Members should be prohibited from having a seat, either, on the Council, or Examining Boards from this unjust restriction; and it is rendered more unfair by the fact that those who were members of Council or Examiners at the framing of this Clause can still be elected to either of these appointments, although they have not the Fellowship Degree.

Several papers have appeared from time to time against the dual appointment, but none of those who hold it have yet deigned to reply to their assailants, although it is a position they cannot treat with the indifference that this line of inaction would appear to indicate. They have left their defence to "M. C." or any one who may lift the pen in their behalf: they appear to consider silence golden, and to wish to have the subject brought before the notice of the profession as seldom as possible.

The defence advanced on their behalf, by M. C. has already been completely demolished by "Elector." It is true, as stated by M. C., that the Council elect the Examiners, but he should have also added, that the number of Examiners on the Board greatly assist in electing each other. According to M. C., their advice will also be taken as to who should be elected as Examiners outside the Council. I believe every one will be satisfied with those gentlemen who have been elected as Examiners, and that they are well qualified to examine upon the subjects for which they have been selected. If these gentlemen when they were elected as examiners had retired from the Council Board, it would have been more satisfactory to the profession. For so long as Examiners retain their seat on the Council, it cannot but be expected that they will exercise an influence, more or less, on subjects pertaining to their own interests, and also by their numbers on the Board they will be able to carry almost any measure they wish.

I have no desire to drag up the past, but not so long ago there were certain inquiries to be instituted by the Council, which, so far as I am aware, never took place. I have no doubt had Examiners not been on the Council such inquiry would have had a better chance of taking place.

It would also be worth while knowing the opinions of M.C's. colleagues regarding his production in favour of the dual appointment, and if they also are of opinion that their services cannot be dispensed with, and that without their aid the business of the Board could not be as efficiently performed,

OPERATIONS FOR ROARING.

By Dr. R. H. Clarke.

I did not at first think it would be necessary for me to take any notice of Dr. Fleming's attack in the current number of the *Veterinary Journal*, as the misstatements, and the source of the strong personal animus he exhibits must be sufficiently obvious to those who are acquainted with the subject, and have read the paper which is the principal object of his attack. It has been impressed upon me however by others: 1—that many people may only see Dr. Fleming's version, and might misunderstand my silence, not a very important matter, and 2—that the subject is one of rather more than mere personal

interest, and therefore, though I would much rather have avoided a personal controversy on what should be a scientific subject, I am sending you some comments on Dr. Fleming's article for publication if you think it worth while to find space for them in your paper.

1.—Dr. Fleming writes:—In *The Journal* for last "July in an article on larygneal surgery we referred "to what we had done in operating directly upon "*that organ*," (sic). It is needless to say the plural pronoun is editorial and does not necessarily imply plurality of person, and it is instructive to compare this with a letter which appeared in the *Standard* of Oct. 13th, last, signed by George Fleming, in which he says: "I have not within the last five "years performed any operation on horses for the "relief or cure of roaring." On referring to *The Journal* for last July as advised, I find an editorial article on Larygneal Surgery, and a paper by Professor Smith giving details of several larygneal operations. Searching the former for some information of "what we had done," the following is all I can find—"For a considerable period we have en- "deavoured to devise a plan of operation which "while much simpler, easier and more likely to be "effectual than any yet attempted, would be attended "with less mutilation and subsequent danger. This "method is described by Mr. Smith in the present "number of *The Journal* though the description is "not quite exact, as the manual details, and indeed "the entire operation, have been considerably modi- "fied." Of the nature of these modifications there is no information. From this ingenious attempt to "sit on the fence" as the Americans term it, I turn to Professor Smith's article, where at least one finds something better than oracular ambiguities and "hedging." In this paper Professor Smith says:— "Whatever subsequent benefit to the profession "these notes may be, the credit must be given to "Dr. George Fleming, under whose immediate "superintendence some of the operations have been "performed, and through whose professional enter- "prise the enquiry has been undertaken." As he receives the credit, Dr. Fleming must be expected to share the responsibility.

The first operation Mr. Smith describes is excision of the left vocal cord. The larynx is opened by a median incision extending from the Thyroid cartilage to the first ring of the trachea, the left vocal cord is transfixed with a curved needle and excised with a hernia knife and a pair of curved scissors. This, Mr. Smith tells us is Fleming's operation, so called after the originator, Dr. Fleming. That the results of this operation were such as to make Dr. Fleming wish to "modify the whole operation" I can well believe, and the only thing that surprises me is that "with his intimate acquaintance with the literature of the subject" he should ever have adopted it. But the point is, that, unless Professor Smith is guilty of an amount of misrepresentation that is absolutely incredible, this was, up to the date of Mr. Smith's paper, undoubtedly Fleming's operation, and it was on the strength of having discovered it that he obtained credit for "the greatest achievement we have ever had in veterinary surgery." Dr. Fleming may be expected to know that K. Gunther practiced excision of the left vocal cord twenty-five years ago, and his "friend and worthy colleague," Professor Möller, or Moeller or Mœller, if he prefers it,—says that Beuley's experiments in 1868 induced him to repeat the excision of the vocal cord, an operation described by Goubaud in 1869, and adds:—I operated with the same results as Gunther. What but the knowledge that his first position was untenable could have driven Dr. Fleming to the hopeless expedient of trying to persuade us that he was the founder of laryngeal surgery, and had discovered the fact that it was practicable to open the larynx at all! and make this assertion, "until the publication of our proceedings "this year no one had ventured to open the larynx, "at least in this country, with a view to performing "operations on its interior." In my paper there is a full description of the first operation performed by Professor Horsley, May 14th, 1887. The larynx was opened in the middle line, the left cartilage of Santorini excised and the left cord divided—two months later the horse was killed, his larynx, which I have kept, was dissected, and a very good drawing of it published in my paper. As Dr. Fleming minutely criticises my description of the second operation in the same paper, he can hardly expect anyone to believe that he did not know of the first. The larynx had however been opened before this by Gamgee, and probably others in this country, and repeatedly abroad by several operators, including K. Günther, Gerlach, Stockfleth and his pupils, and Möller, and in the face of this what are we to think of Dr. Fleming's statement: "Certainly we "may lay claim to being the discoverers of this new "field for operative interference;" or the foundation on which this modest claim is based viz., an attempt to remedy the fractured thyroid of an aged horse in 1878, resulting in the astonishing discovery that the larynx of a horse could be opened without killing him, a discovery so valuable that Dr. Fleming was enabled after ten years reflection to recommend a colleague to repeat an operation that had been amply tested and discredited before. It is a small compliment to the veterinary profession to assume that they shared the superstitious awe with which Dr. Fleming regarded the interior of the larynx, and I decline to believe they did so. It certainly never occurred to Mr. Horsley or myself that there was any danger or difficulty in opening the larynx, though we had neither of us heard the story of the aged horse.

As a last resort to sustain his failing credit as a discoverer Dr. Fleming maintains that no previous operators opened the larynx at all; but excised the vocal cord by an incision into the trachea—"a long way from the head" (sic). When the object of the operation was to excise the vocal cord, it was not a matter of great importance, so long as the vocal cord was excised, whether the opening through which it was done was an inch or so higher or lower, but if anyone excised it from an incision entirely below the cricoid, he must have exercised some quite superfluous dexterity. I am inclined to think this assertion as unfounded in all cases

as it undoubtedly is in some. Apart from Möller, who distinctly states that he did divide the cricoid in some of his operations, it is improbable that any operator having divided two or three rings of the Trachea should, without any reason, abstain from dividing the cricoid, which is practically a tracheal ring like the rest in front, and so gratuitously create a difficulty in reaching the vocal cord, and incredible that Stockfleth performed his operation viz., excision of the upper part of the arytenoid cartilage from an incision entirely below the cricoid, and I doubt whether this is possible.

There is another discovery however, which enabled Mr. Smith to recommend a modification in the operation of excision of the vocal cord, which has certainly never been previously performed by any surgeon British or Foreign, this is the recommendation to reflect the mucous membrane in flaps from the surface of the vocal cord before excising it:—there is only one objection—it is not possible. There is no submucous tissue over the cord, and the mucous membrane is so intimately adherent to it as practically to form a single structure, and dissecting off the mucous membrane involves dissecting off the cord with it.

I fancy most surgeons are familiar with this fact, which has a practical bearing in relation to œdema of the larynx, the effusion always stopping short of the region of the vocal cords. A busy practitioner might forget such things; but a Professor of Surgery, and a translator of Chauveau's Comparative Anatomy should have avoided such an impracticable suggestion. Dr. Fleming's personal attentions to myself are of little consequence; but afford further illustration of his consistency and general accuracy. He sneers at me for having *ventured* to transfer some of my attention from mankind to horses—and a few pages further on, in an address delivered at the Veterinary College he deplores the want of interest the medical profession has exhibited in the progress of Veterinary medicine, contrasting its conduct in this respect unfavourably with that of former surgeons, rejoices at signs of improvement in this attitude, and says the two professions are one. He says that not long ago, before the publication of my paper, I announced that I had cured two horses Where is this statement to be found? Still referring to me he continues: "Fearing evidently that we might gain credit for our attempts, he subsequently in a military Journal! published his observations!!" If it gratifies Dr. Fleming to think that I only published my experiments through jealousy of him, I don't grudge him the satisfaction, any more than I do any credit he deserves. Those who recollect the dignified reserve he displayed in communicating his discovery to the world will appreciate the note of exclamation after Military Journal. It is hardly necessary for me to explain my reasons for selecting that channel of communication, until I know of some rational objection to it. I might point out however, that I have no professional obligations in connection with the subject of roaring, in horses, that I am not the Editor of a Veterinary Journal, that I have not advanced any claim to the discovery of an operation that has been done before, and I did not lead the public to believe that I had discovered a cure for roaring a fortnight after my first operation.

"We glean that Mr. Clarke's operation consists of first opening the trachea to ascertain whether the horse is a roarer." Dr. Fleming has gleaned with a reaping hook sometimes; but he does me too much honour in connecting my name with this operation. I have performed a variety of operations, several of which are detailed in my paper; but I have no proprietary interest in any of them. I never opened a horse's trachea to ascertain whether he was a roarer, I never said I did, and I never said anything that conveys such a meaning.

In my description of the operation which Dr. Fleming quotes "jugular" vein should have been "glosso-facial." This alteration was made in the copies that passed through my own hands; but overlooked in a hasty correction of proofs. In one operation to the best of my recollection I did tie the jugular; but not as a rule, and with the incisions described it does not cross the wound. This was rather a good opportunity for Dr. Fleming if he had noticed it.

Dr. Flemtng refers with the greatest contempt to the tests I employed, and to the weakness exhibited in such phrases as "I *think* they were cured." After making all due allowance for playful exaggeration, no one could pretend that a horse's wind could be tested at the Brown Institution as satisfactorily as by a good gallop in the open; but it is quite as incorrect to represent the former test as entirely worthless.

We found it easy to detect the horses which were roarers when we got them, and I will venture to say, that if anyone cares to send a batch of horses, roarers and others, to the Brown Institute, they will be distinguished. Before commencing my experiments, I endeavoured to procure a roarer, and having heard of one in the country, I purchased him and arranged with Mr. Busby to receive him at the Brown. I had not seen the animal and believing that he was a roarer told Mr. Busby so; but before sending the horse up I gave him a gallop, and found he was not a roarer at all; still I sent him without saying anything more. When I arrived at the Brown, I found that they had been trying him, and Mr. Busby told me that I had been deceived, as the horse was not a roarer. I might add that even a less reliable method would acquire some value as a differential test, if it indicated,as this did, marked differences in an animal before and after the operation, as a thermometer, which cannot be relied upon for absolute temperatures, may serve well enough to indicate differences. The inspection of the larynx of a horse under chloroform, and subsequent dissection of the parts afford corroborative evidence of the results of an operation; and lastly, when I have stated my conclusions, I have endeavoured to explain the steps by which I have reached them. A point in respect of which I don't think I have much to learn from Dr. Fleming. It seems scarcely worth while to try and advocate the advantages of experimental operations undertaken with

the object of ascertaining what would occur, rather than the hope of at once obtaining the best possible results in the best possible way, and of which I am not sanguine enough to think that any one is likely to be "the operation of the future." But as far as I can judge, extra laryngeal excision of the arytenoid as I have described it, occasions far less mutilation, and is much better surgery than any form of intra laryngeal excision, partial or complete, involving injury to the mucous membrane, and of the evil consequences which Dr. Fleming assumes will follow, I may say with Professor Smith they do not occur. In laryngeal, as in other surgery, the scalpel should count for more than the pen or the trumpet, and those who have made practical contributions to this subject will remember that Dr. Fleming "has performed no operation for the relief or cure of roaring for the last five years," and none apparentiy worth mentioning before that time, except the attempt ten years ago to mend the fractured thyroid of that aged horse, whose pardon I should like to ask for trotting him out once more, in the midst of his strenuous efforts to drag the labouring chariot of discovery. I have already trespassed too much on your indulgence and will only add that I commenced the description of my experiments by saying that I had no intention of publishing them at their present stage, a remark which appears to indicate that I did not regard them as final or conclusive; I explained that "my object was to test experimentally various apparently practicable methods of relieving roaring in horses, to perform the operations which appeared best calculated to effect this purpose, and after keeping the animals long enough to be able to estimate the ultimate results of each experiment by examination of the conditions which resulted during life and after death, by repeating also in several cases the most satisfactory of these operations, with any modifications which suggested themselves, I hoped to be able to offer some reliable data to those whose time and opportunities prevented their undertaking the work of investigation themselves." I expressly disclaimed "anything approaching the dignity of a discovery," and described as well as I could the operations and their results. This preliminary stage afforded me information which I was anxious to acquire, and which I thought might be interesting and perhaps useful to others; but I should probably have deferred publication to a later period, but for circumstances which have been sufficiently discussed.

ON SOME OF THE

COMMUNICABLE DISEASES OF MAN AND THE LOWER ANIMALS

WITH REMARKS ON THEIR ORIGIN AND RELATION

By Edgar Crookshank, M.B.

From a paper at the Annual Meeting of the Midland Branch of the British Medical Association, held at Nottingham, June 14th, 1888.

I propose to bring to your notice some of the diseases communicable from the lower animals to man, and to discuss certain points in connection with their origin and relation; and I shall also deal with those diseases in man, which have been *alleged* to arise from diseases of the lower animals. The diseases to which I refer I shall divide, for the purposes of discussion, into three groups. First, I shall draw your attention to some communicable diseases which are transmitted to man, but do not arise in man independently of the lower animals, such as cow-pox, foot and mouth disease, anthrax, and glanders.

Cow-pox.—Jenner was the first to describe the disease which had been known in the dairy counties from time immemorial as the "cow-pox," and his publications lead to a careful study of the diseases affecting the teats of cows.

I need hardly say that as this disease is one of the sources of the lymph employed for affording protection against small-pox, it is of immense interest and importance. It is therefore much to be regretted that the clinical history of the natural disease in the cow has been, until recently, so much neglected in this country both by the veterinary profession and our own. It is commonly supposed that both cow-pox and horse-pox have been absent from this country during the long period succeeding the outbreaks described by Jenner, Loy, Ceely and Estlin; but inasmuch as outbreaks have been comparatively common during this period in other countries, it is far more probable that outbreaks have occurred in England, but have been concealed, overlooked, or wrongly diagnosed. I shall again refer to this disease in speaking of the alleged origin of small-pox. I shall therefore now pass on to describe another vesicular disease affecting the cow's teat—of especial interest to us, as the virus is conveyed by the milk.

Foot and Mouth Disease.—This is a highly contagious and infectious febrile disease, characterised by a vesicular eruption affecting the lips, tongue, roof of the mouth, and the feet of sheep, cattle, and pigs. The milk is contaminated by the discharge of the vesicles, and is unfit for use, either as food for the human being or for the lower animals. It induces a vesicular eruption in the mouth, larynx, pharynx, and intestinal canal. It acts most vigorously when administered warm to young animals, and calves occasionally die quite suddenly after sucking cows affected with the eruption on the teats. Fatal effects also result when the milk is administered to young pigs.

It has been stated that no injurious consequences arise from the consumption of the milk by human beings, but there is abundant evidence to the contrary, and the conflicting opinions probably arise, as Brown has pointed out, from the fact that milk is seldom drunk direct from the cow, and rarely in an undiluted form. Hertwig experimented upon himself with milk freshly drawn from a cow with the eruption. He drank a pint, and two days afterwards experienced slight fever, restlessness, and headache. The mouth was dry and hot, and there was tingling in the skin of the hands and fingers. These symptoms continued for seven days after taking the milk. On the ninth day vesicles had formed on the tongue, principally on the edges,

and on the mucous membrane of the cheeks and lips (the largest being about the size of a lentil). They were yellowish white in colour, and contained a whitish turbid liquid, which flowed when the vesicles were pricked with a needle. At the same time a number of vesicles developed on the hands and fingers; the greater part of them at the time of their first appearance were the size of a millet seed. They were firm to the touch, yellowish white, and occasioned a slight tingling. On the lips the vesicles dried up, and were covered with thin brownish crusts, which fell off ten days after the appearance of the first vesicles. The vesicles which developed on the hands ran a slower course. From the tenth to the thirteenth day they filled with a liquid, like turbid lymph. They were large and confluent, and finally broke and dried up.

Glanders is a malignant contagious and fatal disease, occurring in the horse, ass, and mule, and occasionally communicated to man.

Anthrax is a disease affecting cattle, sheep, and horses—and, I must add, swine. Toussaint in France is responsible for the doctrine that swine are insusceptible, an opinion also maintained by Klein. During the past year I have made a series of researches, which have established the fact that anthrax can be communicated to pigs, both young and old.

The disease is also communicable to man. Most frequently it results from inoculation of some lesion or abrasion of the skin. Hence it is by no means uncommon in butchers who have been dressing an animal which has died from anthrax, or in veterinary surgeons who have been making a post-mortem. In such cases the disease is more or less local. In one case brought to my notice, a veterinary surgeon made a post-mortem upon the body of a bullock that had died the night previously. The spleen and parts of the viscera were forwarded to me for examination. The blood from the spleen was full of the characteristic bacilli. As I was informed that the veterinary surgeon had inoculated his finger, I wrote for particulars, and ascertained that the bullock had been found dead in a meadow near a river. After making a post-mortem Mr. A—— wiped the blood off his hands with some rough grass, which cut the skin on the inner side of the third finger. The hand was well washed in the river, and a strong solution of carbolic acid applied. On the following night there was a tingling sensation in the finger, and the next day it became considerably inflamed, and a distinct pustule had formed, with several smaller pustules surrounding the larger one. Mr. A—— became restless, and at times partly delirious, and if he attempted to sleep these feelings were much intensified. He also suffered from great mental depression, which continued during the attack. On the fourth day he lanced the pustules, and let out the contents. The finger was much swollen, hard, and extremely painful, and the lymphatics of the arm were inflamed. On the sixth day a medical man was called in, and caustic applied. The disease progressed, the pustule increased in size, and on the twelfth day the finger had become black, cold, and much swollen, and the pain greatly intensified. A surgeon was called in, and the finger amputated; the pain then subsided, the inflammation of the lymphatics began to disappear, and he gradually recovered.

SECOND GROUP.

This comprises communicable diseases in man, which may *possibly* be derived from similar diseases in the lower animals.

Tuberculosis is extremely common in cattle and poultry, and also affects pigs, and more rarely horses and other animals. As numbers of cattle slaughtered for the market are tubercular, and as cows with tubercular udders are occasionally found in dairies, it becomes a serious question whether human tuberculosis may not in some cases arise from the consumption of meat and milk containing the virus of bovine tuberculosis. Animals, such as pigs, rabbits, and fowls, if fed on tubercular material rapidly become infected. Is it possible that the flesh of tuberculous animals may infect man? We can only say that direct evidence of the communication by such means to man of bovine tuberculosis is at present wanting. This may be owing to the fact that as meat is cooked, the virus is destroyed. But if it is possible for this transmission to occur through uncooked material, it would naturally be expected that bovine tuberculosis would be conveyed by milk, which as Bang has pointed out, is seldom boiled. The possibility of this source of tubercular infection has been especially brought forward of late by the researches of Bang, and following in his footsteps, by Woodhead and McFadyean. The milk of cows, with tuberculosis of the udder, contains tubercle bacilli, and such milk given, either by the mouth or subcutaneously, to other animals produces tuberculosis. On six occasions they were able to examine 623 cows, kept by thirty-seven dairy farmers. In fifteen of these dairies they found the whole of the stock healthy. Amongst the others were thirty-seven beasts, in which there were more or less marked changes in the udder, and in six cases they were able to demonstrate the presence of tubercle bacilli.

I have had the opportunity of examining two cases of udder tuberculosis, and have been able to confirm the observation of these and other investigators with regard to the presence of tubercle bacilli in the milk. This milk, when administered with other food, produced in rabbits tubercular ulceration of the intestines and tuberculosis of the mesenteric glands. I found that in such cases the bacilli could be more readily demonstrated by collecting the milk in test-tubes, and setting them aside for a few hours. The milk is then carefully poured away, and the sediment at the bottom of the tube spread on a cover-glass, and stained in the ordinary way.

In both these cases the milk had been mixed with the general supply of the dairy. There is thus ample evidence that the milk of tuberculous cows finds its way into the market, but what evidence is there of its producing tuberculosis in the human subject? If such did occur, primary tuberculosis of the intestines might well be expected to

be a common affection, but such we know is not the case in the adult. This would, therefore, tend to contra-indicate danger to the adult, but on the other hand Woodhead has emphasized the probable danger to children. Woodhead has pointed out that from his experience in two large hospitals he has been much struck by the fact that in children who had succumbed to other diseases during the course of tubercular disease of the abdominal glands, there was frequently not any trace of tubercular disease in other parts, thus pointing to the intestine as the channel by which the bacillus made its way into the body. Woodhead also remarks that in a large number of cases of general tuberculosis, where the possibility of infection by the pulmonary passages was evidently excluded, the tubercular process appeared to have invaded the body by the intestinal canal.

Actinomycosis.—This is a disease of cattle which has long been reported on the continent as a common disease, but until somewhat recently in this country it was only regarded as of occasional occurrence, affecting the jaws and tongue, and rarely the nostrils and pharynx. My attention has been drawn to this disease for some years, but I will only briefly refer to some facts which I have experienced during the past year. In June, 1887, I had the opportunity of examining a black heifer, three years old, with a large growth in the parotid region. This tumour was discharging in places, and on firm pressure a quantity of yellowish muco-purulent matter was squeezed out, and collected in test-tubes. The case was reported to be one of several affected with the same disease. On spreading some of the discharge in a film on a slide, I observed the little grains which to the naked eye recalled the appearance of actinomycosis, and on covering with a cover-glass, and examining first with a low and then a high power, the characteristic tufts and club-shaped structures were revealed. The animal was killed, and a section through the growth revealed the characteristic naked-eye appearances of an enormous actinomycoma. Further inquiries were set on foot, and I have been supplied with a large number of cases from different parts of the country, while quite recently I have been investigating two outbreaks of actinomycosis where the disease has somewhat suddenly appeared in a considerable number of animals. There is no doubt then, that in this country actinomycosis is extremely common, and that it is particularly prevalent in certain low-lying districts. If then the affection is communicable to man, and bears any relation in frequency to the cases in cattle, numerous cases must have been overlooked by the pathologist, for only a few cases of a disease described as actinomycocis in man have been published in this country. The existence of such a disease in man is now well established, and continental observers have placed nearly 100 cases on record. But out of 75 cases collected by Moosbrugger only ten individuals were farmers, peasants, or farms labourers, and two only were in direct contact with cattle. And in connection with this we must bear in mind that the micro-organism associated with the disease in cattle, differs in details from the micro-organism in the disease in man. These differences are very marked, but they may possibly be accounted for as the result of the same micro-organism growing under different conditions of soil. On the other hand, in the present state of our knowledge it may be urged with equal force that, though similar in some respects, we are in reality dealing with two distinct micro-parasitic affections, which occur quite independently of each other.

THIRD GROUP.

This comprises a number of diseases which have been alleged to have their origin in diseases of the lower animals. The theory of the animal origin of the specific poisons of infectious fevers in man has been brought prominently forward in the alleged origin of scarlet fever from a disease of the cow, and this has been discussed with so much gravity that many have been lead to suppose that it is an entirely new idea which may revolutionise our knowledge of the pathology and preventive treatment of these diseases. I shall endeavour to show you that the theory dates back to the earliest history of medicine, and that it has been revived from time to time, only to be refuted and relegated to the domain of exploded ideas. We first hear of it in connection with the origin of small-pox.

Small-pox, we are told, was attributed by the Arabians to a disease of the camel, and this belief was regarded as an argument in favour of its origin from the lower animals, when Jenner promulgated a similar idea. But Jenner substituted the horse for the camel. He considered it probable that small-pox in man arose from morbid matter of a peculiar kind, generated by horses suffering with "greasy" heels. Baron informs us that this idea was prevalent in Jenner's mind as early as 1787. It is related that in that year he accompanied his nephew, George Jenner, into a stable to look at a horse with diseased heels, and, pointing to them, he remarked: "There is the source of small-pox. I have much to say on that subject which I hope in due time to give to the world."

Jenner's views have not found favour. It has been shown by Loy and others that the grease bears no relation to cow-pox, and it is now generally believed that Jenner mistook a vesiculopustular disease, the same disease in the horse as cow-pox in the cow, for the disease known as the grease. This vesiculopustular disease of the horse, previously described by veterinarians under various names, is now called horse-pox. However, this theory of the origin of the specific febrile diseases was not allowed to rest; another infectious fever in man was stated to have its origin in a disease of the horse.

Scarlet Fever.—Dr. Copland, in his "Medical Dictionary," states that scarlet fever in man was originally a disease of the horse, and that it formerly occurred, and has recently occurred, epidemically as an epizootic amongst horses; secondly, that it was communicated in comparatively modern times from horses to man; thirdly, that it may be, and has been, communicated to the dog. But this

opinion has not been accepted, for the disease known as scarlatina in the horse is described as a non-infectious disease, and Williams states that many cases of the so-called scarlatina of the horse are in reality identical with purpura.

And now, comparatively recently, the theory has again been revived, and finally has been adopted by Dr. Buchanan and the Medical Department of the Local Government Board.

In 1882 an epidemic of scarlatina in St. Giles and St. Pancras, was investigated by Mr. Power for the Board. The disease was distributed with a milk supply from a Surrey farm. In this case two facts were ascertained, the one that a cow recently come into milk had been suffering from some ailment from the time of her parturition, of which loss of hair in patches was the most conspicuous manifestation. The other that there existed no discoverable means by which the milk could have received infective quality from the human subject.

In 1885 another outbreak of scarlet fever occurred in connection with milk from a farm at Hendon, and again Mr. Power failed to establish infection from any human source in any commonly accepted way—such, for example, as handling of milk, or milk utensils, by persons carrying scarlatina infection. But on examining the cows with a view to ascertaining any new condition pertaining to them, it came to light during the inquiry that some of them, which had been recently introduced from Derbyshire, were suffering from a vesicular disease of the teats. Dr. Cameron reported on the clinical history of the disease, which was communicable to the hands of milkers, while Dr. Klein brought forward pathological evidence to support the theory of bovine scarlatina. Dr. Klein found a streptococcus in the discharge of the ulcers on the teats, and in some of the cows he found lesions which were alleged to recall the lesions of scarlet fever. A similar streptococcus was found in certain cases of scarlet fever in man, and the microbes from either source, when inoculated *en masse*, produced similar lesions in calves. It was therefore concluded that the Hendon cow disease was scarlet fever in the cow, and that scarlet fever in man originated from the consumption of the milk of cows suffering from this disease. Now, so far as pathological evidence goes, I have pointed out, in my papers read before the Pathological Society of London, that these data admit of an entirely different interpretation. The post-mortem lesions were those of septic complication, and common therefore to many diseases, and the streptococcus is a septic micro-organism occurring as a secondary result in many affections besides scarlet fever. Inasmuch as the streptococcus and the septic discharge from ulcers produced on inoculation in cows certain post-mortem appearances, which are common to many diseases, and failed to produce fever, ulceration of the tonsils, or scarlatinal rash, or any condition in the least resembling clinically the disease in man, the conclusion that the result was scarlatina in the cow was quite unjustifiable; and if you take the trouble to study the description of the clinical history of the disease by Dr. Cameron, and the *local* result obtained by Dr. Klein after inoculating calves with septic lymph from the cows' teats, resulting in spurious vaccination, you will fully understand the arguments which I have brought forward in support of the opinion held at the Hendon farm, that the disease was, in reality, cow-pox—an opinion strengthened by the complete correspondence with an outbreak of cow-pox in Wiltshire, the account of which I shall shortly publish in full. That this disease was merely coincident with the contamination of the milk is borne out by the fact that at the same time the same cow disease was introduced from the same herd into other farms, where it was communicated in the same way to the healthy cows and to the hands of the milkers, and yet the milk supplied from these cows was freely consumed without producing any scarlet fever.

Diphtheria has also been supposed to arise from diseases of the lower animals. Dr. Cameron, after giving his account of the Hendon cow disease, says: "I am not assuming that this cow malady, under discussion, is capable of causing in the human subject any other disease than scarlet fever, but I regard it as helping to throw some light upon the natural history of diphtheria. Indeed, there may be an, as yet, undiscovered disease perhaps, without much skin eruption, and without conspicuous disease of the teats or udder, and probably also of a trivial nature in the cow with symptoms somewhat different from those of diphtheria in man, which is capable of causing this disease in human beings through the medium of the milk from such cows. At the same time it is possible that diphtheria may be caused by some modification of this particular malady."

Dr. Turner has also suggested the origin of diphtheria in man, from certain diseases simulating diphtheria in cats, pigs, and horses. I need add nothing to these descriptions which, indeed, speak for themselves as to their purely hypothetical character, and I fear that they have been brought forward owing to a tendency to pursue fanciful theories, which are liable to conceal our ignorance on the subject of the origin of these diseases, and regardless of the damaging result of their application by a public only too apt to listen to the voice of alarmists. I have endeavoured to lay before you the dangers, real and imaginary, with regard to the transmission of some of the diseases of the lower animals to man, and place both sides of the question before you. I would have you sift the evidence carefully before you formulate conclusions, and above all not be too ready to blame the sister profession if your conclusions are not accepted. On the contrary, when we consider that small-pox has been asserted to arise (*a*) from a disease of the camel; (*b*) from grease in the heels of the horse; (*c*) from vesicles on the cows' teats: and that scarlet fever has been asserted to arise (*a*) from purpura in the horse; (*b*) after parturition in the cow; (*c*) from vesicles on the cows' teats: and that diphtheria has been supposed to arise (*a*) from garget or mammary abscess; (*b*) from a disease like cow-pox; (*c*) from a disease in the cow without conspicuous eruption; (*d*) from a disease in the cow without any con-

spicuous symptoms; (*e*) from diseases of cats, pigs, and horses, simulating diphtheria, I think that pathologists will rather sympathise with any apathy that may be displayed by the veterinary profession in listening to the opinion of the theorists of our own.

GOVERNMENT VETERINARY DEPARTMENT.

CONTAGIOUS DISEASES IN ANIMALS.

Reported in Great Britain in the two weeks ending November 10th 1888.

Pleuro-Pneumonia.—There were 12 fresh outbreaks of this disease reported during the fortnight in Great Britain; 8 in England, in the counties of Essex, Kent, Norfolk, Somerset, Sussex (East), and York (W.R.); the other 4 were in Scotland in the counties of Edinburgh and Fife.

The number of cattle attacked by the disease was 72, of which 50 were in England and 22 in Scotland. In looking over these returns as they are published in the London Gazette, it seems rather strange at first that 21, or, nearly half of the diseased cattle in England should be in the Metropolis, and 7 or, nearly a third of those in Scotland, in Lanarkshire, districts in which no fresh outbreaks of the disease had occurred, but the explanation of this probably is that in slaughtering the whole of the animals in a herd which had been exposed to infection, these cases were discovered in cattle which were not suspected and had probably never shown any obvious symptoms of the disease.

Of cattle which had been in contact with the diseased, or otherwise exposed to infection, 296 were slaughtered, 229 in England and 67 in Scotland.

In Ireland 4 fresh outbreaks were reported in the Unions of North Dublin, South Dublin, and Rathdown. There were 4 cattle attacked and 98 healthy which had been exposed to infection slaughtered.

Anthrax.—There were 6 outbreaks of this malady reported in England during the fortnight and 21 animals attacked by it. Scotland and Wales had no fresh outbreaks nor animals attacked.

No cases of this disease reported in Ireland during the fortnight.

Swine-Fever.—Of this disease 200 fresh outbreaks were reported and 1,254 swine were affected, 534 or less than half of which were killed; 492 died, 50 recovered, while 408 of the diseased pigs remained alive as active centres of disease at the date of the last published returns.

In Ireland there were 8 fresh outbreaks of swine-fever reported and 27 pigs were attacked by it. Of these 9 were killed and 9 died.

Glanders and Farcy.—There were 28 fresh outbreaks of glanders reported in the fortnight, all of them in England. No less than 24 were in the Metropolis, while of the remaining 4 there were 2 in Middlesex, 1 in Surrey, and 1 in East Sussex. Of the 48 horses attacked with glanders during this period, 38 were in the Metropolis, 2 in Middlesex, 1 in Surrey, and 1 in East Sussex, and 6 in Lanarkshire.

There were 22 outbreaks of farcy reported in the same time. 20 of which were in the Metropolis, and 2 in Middlesex; while of the 28 horses suffering from this disease, 25 were in the Metropolis, and 3 in Middlesex.

No cases of either glanders or farcy were reported in Ireland during the two weeks above referred to.

Rabies.—There were 4 cases of rabies in dogs reported in England during the fortnight, 2 of these were in the Metropolis, 1 in Lancashire, and 1 in Surrey.

In Ireland 10 cases of rabies were reported, the animals affected were 5 dogs, 4 cattle, and 1 horse.

EXTRACTS.

SWINE FEVER.

On the 12th instant, at the Blofield Petty Sessions, E. Gilbert, Esq. in the chair. William Wesley Key, farmer and dealer, Southwood, was charged under the Contagious Diseases (Animals) Act, with having in his possession certain swine affected with swine-fever, and neglecting to report the same to the police as required. Superintendent Basham deposed hearing that defendant had swine-fever on his premises. He communicated with Mr. J. D. Overed, the Inspector for the district, who accompanied him to Key's farm, and found the disease to be present.

Mr. Overed deposed that he visited defendant's farm at the request of Superintendent Basham on the 29th ult., and ascertained that five pigs had died, and were buried on the premises, these he ordered to be disintered, and on examination he found unmistakable evidence of swine-fever.

He next examined five living pigs, which were hurdled off in a field about 80 yards from the farm premises, four of which were found in an advanced stage of the disease, these he ordered to be slaughtered, and together with the carcases of the five dead pigs, to be buried in lime, as required by the Act.

In reply to the Chairman, Mr. Overed stated that he did not receive notice from the defendant that the disease was on the farm.

Defendant in answer to the charge replied, that he was not aware of the nature of the disease, or he should not have brought the pigs on to his premises where he had fifty fat pigs worth £150.

The Bench considered the case proved, and fined the defendant £5, and £1 14s. 0d. costs.—'*Norfolk Chronicle.*'

Infringement of the V.S. Act.

At the Westminster Police Court, on Thursday, November 8th, before Mr. D'Eyncourt, F. H. Dale, Brompton Road, was charged with an offence under Section 16 of the "Veterinary Surgeon's Act, 1881." The defendant pleaded guilty, and was fined in the mitigated penalty of ten shillings.